POPULAR ASTRONOMY.

POPULAR ASTRONOMY:

A SERIES OF LECTURES.

BY

GEORGE BIDDELL AIRY,

ASTRONOMER ROYAL.

LONDON AND CAMBRIDGE:

MACMILLAN AND CO.

1866.

IPSWICH:
COWELL, BUTTER MARKET.

PREFACE TO THE FIFTH EDITION.

I AM informed by the Publishers that this Fifth Edition is required to meet the demand of a somewhat wider class of students than those for whom the Lectures were originally intended.

This circumstance, as well as the advances made in Astronomical science during the period which has elapsed since the Lectures were delivered, have rendered it desirable that some very slight modifications should be made in the text, and that some additions should be made in the form of Appendix.

I am happy to state that Mr. Stirling has been at liberty to prepare the modifications and additions

to which I allude, and to undertake the general editing of the book. And I now issue the work to the public, with the increased confidence derived from the assistance of a friend on whose ability I place complete reliance.

G. B. AIRY.

ROYAL OBSERVATORY,
GREENWICH, 1866, *June 9th.*

INTRODUCTION.

In conversing with persons who are not officially attached to Observatories, or in other ways professionally cognizant of the technicalities of practical Astronomy, but who, nevertheless, display great interest in the science of Astronomy generally, I have frequently been struck with two remarks. The first is, that these persons appear to regard the determination of measures, like those of the distance of the Sun and Moon, as mysteries beyond ordinary comprehension, based perhaps upon principles which it is impossible to present to common minds with the smallest probability that they will be understood; if they accept these measures at all, they adopt them only upon loose personal credit; in any case, the impression which the statement makes on the mind is very different from that created by a record of the distance in miles between two towns, or of the number of acres in a field. The second remark is, that when persons well acquainted with the general facts of Astronomy are introduced into an Observatory, they are, for the most part, utterly unable to understand anything which they see; they are impressed perhaps with the apparent complexity of subsidiary parts of the Astronomical instruments, and they imagine that the fundamental

principles of their construction are complicated, and too obscure for the understanding of ordinary men; and they leave the Observatory without having derived from it any clear idea whatever.

In both cases, however, the difficulties are very much over-estimated: or rather, difficulties are assumed which, in reality, do not exist at all. The measure of the Moon's distance involves no principle more abstruse than the measure of the distance of a tree on the opposite bank of a river. The principles of construction of the best Astronomical instruments are as simple and as closely referred to matters of common school-education and familiar experience, as are those of the common globes, the steam-engine, or the turning-lathe; the details are usually less complicated.

In the application of the ordinary principles of geometry and trigonometry to such Astronomical measures as those to which I have alluded, it may sometimes be expedient to resolve the process into several successive steps, and these steps may perhaps require different kinds of treatment. But the remark which I have made applies to every individual step; all are simple and within ordinary comprehension, and the only complexity arises from the circumstance that the student may find it necessary to have a clear view of several such steps at once, in order to perceive the connection between the first standard of length and the numerical measure last obtained.

With these impressions on my mind, I had long wished for some opportunity of endeavouring to explain to intelligent persons the principles on which the instruments of an Observatory are constructed, (omitting all details, so far as they are

merely subsidiary), and the principles on which the observations made with these instruments are treated for deduction of the distances and weights of the bodies of the Solar System, and of a few stars, (omitting all minutiæ of formulæ and all troublesome details of calculation). To attempt to go further than this would, in fact, amount to undertaking a complete work on Astronomy, which was far beyond my intentions.

Such an opportunity appeared to present itself in the course of Lectures which I engaged to give to the Members of the Ipswich Museum and their friends.* And the ideas which I have enounced above have been carefully kept in view, in the object and in the details of every Lecture.

I have endeavoured, in the first place, to point out how much of the fundamentals of Astronomy may be obtained by the coarsest observation with the unaided eye. And here I would remark, that the science which is thus obtained by personal observations is vastly superior (as far as it goes) to that which is obtained by any other method. The knowledge derived from Lectures is exceedingly imperfect: that derived from careful reading is admirable for its accuracy and fulness, but occupies the mind rather as a train of internal ideas than as a series of consequences deduced from the observations of nature: but that inferred from actual personal observation carries with it a degree of reality and certainty, as the veritable science of external objects, which nothing else can give.

I have endeavoured, in the next place, to show

* The Lectures were originally delivered at Ipswich, on Monday evening, March 13th, 1848, and the five following evenings.

that the instrumental conceptions derived from the use of a common globe are sufficient, in almost every case, for the understanding of the instruments in an Observatory; that the elements which are the subjects of observation with Astronomical instruments, are the same as those with which we are most familiar in the ordinary globe-problems; and that a person who understands the latter can proceed at once with the former.

Afterwards, I have endeavoured to explain that the methods used for measuring Astronomical distances are in some applications absolutely the same as the methods of ordinary theodolite-surveying, and are in other applications equivalent to them; and that in fact there is nothing in their principles which will present the smallest difficulty to a person who has attempted the common operation of plotting from angular measures.

The elucidation of the theory of centripetal and disturbing forces is necessarily less complete. Still it appears probable that a general conception of the nature of the action of those forces, perfectly accurate as far as it goes, and sufficient to preserve the student from the gross errors into which many persons have fallen, may be obtained from explanations like those here offered.

The methods of ascertaining the weight of the Earth and other bodies, are still more difficult of explanation; yet it is hoped that something may be done even in these.

The first conclusion, then, which I would desire to impress upon the student of Astronomy, who enters upon the science with a tolerable understanding of ordinary practical matters, is, that nothing is totally beyond his reach. Complete

knowledge of every theoretical and instrumental detail can only be obtained by those who will devote to them a large portion of their lives; but sound knowledge of the principles of nearly every part, can be obtained by the reasonable efforts of persons possessing common opportunities for general knowledge.

The second conclusion, however, is that, easy of comprehension as are the successive steps of Astronomical investigation, the united succession of all is not to be seized lightly. Let no one think that the problem, for instance, of the measure of the Earth's distance from the Sun, is to be mastered by one reading. Again and again must the student return to it; again and again must he doubt and become convinced; again and again must he trace his own diagrams, and express the reasoning in his own language, before all the troubles (I will not call them difficulties) are overcome. And perhaps one of the most valuable results to be derived from a truly intellectual study of Astronomy is, the habit of keeping up a sustained attention to all the successive steps of a long series of reasonings. Power, and with it dignity, are gained to the mind by this noble exercise.

To those who will venture upon the study of the science in this connected way, I can promise an ample and immediate reward. It is not simply that a clear understanding is acquired of the movements of the great bodies which we regard as the system of the world, but it is that we are introduced to a perception of laws governing the motion of all matter, from the finest particle of dust to the largest planet or sun, with a degree of uniformity and constancy which otherwise we could hardly have

conceived. Astronomy is pre-eminently the science of order.

The immediate object of my Lectures would be obtained, if they should be found to offer some facilities to those who, acting under the inducements to which I have alluded, may endeavour to obtain a connected and accurate view of the principles of Astronomy. But I should think myself highly rewarded if I could believe that the insight into principles thus obtained, would induce any one to enter carefully into its details.

G. B. AIRY.

Royal Observatory, Greenwich.

POPULAR ASTRONOMY.

LECTURE I.

Evidence for the *apparent* Rotation of the Heavens round the Earth.—The Equatoreal.—Refraction.—The Transit Instrument.—The Mural Circle.—Mode of Observing.

BEFORE entering upon the subject of my proposed course of Lectures,* it may perhaps be desirable that I should state, in as brief terms as possible, the views which have induced me to deliver them to the members of this Institution. When it was intimated to me that the offer of the course would be desirable, and when I felt that my compliance would show my good will to the Museum, I could not help thinking in the first place, that I should be in some slight degree departing from the intentions and objects of the Institution, though in the next place, I was certainly inclined to the opinion that such departure would be more imaginary than real. I thought that Lectures on Natural Philosophy would seem to be hardly proper in an Institution intended for Natural History; but still I was convinced that their subjects were so closely connected, that the habits of thought which they induced, and the mode of treating them, were so similar in many respects, that what applied to the

* The circumstances under which these Lectures were originally delivered are explained in the preface.

one would, in a great degree, apply to the other. Indeed, I felt that most persons would be better prepared for the study of Natural History generally, by the study of Natural Philosophy in its various branches, than if they were in ignorance of the latter. But there were other considerations connected with the desire I have entertained to deliver these Lectures, not so much allied to the subject of Astronomy as matters of private feeling. I have been personally long connected, not with the town of Ipswich precisely, but with the neighbourhood. I remember, with gratitude, that the first time I was shown an astronomical object of any great interest, it was exhibited to me by the founder of the mechanical and manufacturing Institution which has now risen to such great importance in the town of Ipswich. It was by the elder Mr. Ransome that I was first shown the planet Saturn, with a telescope manufactured by his own hands. And I may add, that the first Nautical Almanac I possessed, was received as a present from a gentleman then residing in Ipswich, who has now risen to great eminence in the Metropolis as an engineer. From these and other circumstances I was desirous, when the opportunity should occur, of offering to the members of the Museum, or to any other similar body in the town of Ipswich, a course of Lectures on Astronomy.

In offering them to the authorities of the Museum, I made but one remark—that I understood it would be perfectly agreeable to the members of the Institution, and that if such were the case, it would be also exceedingly agreeable to myself, that the regulations for the attendance upon the Lectures should be framed in such a way as to give facilities of introduction to persons concerned in the mechanical operations of

the town. And here I must beg to say, that the alliance between astronomers and mechanics is much closer than it may seem to be at the first view of the matter. Astronomers have to rely very closely upon mechanics for every part of the apparatus connected with their operations. Possibly mechanics have derived something from their connection with astronomers; but at all events, I am certain the debt is on the other side. I may adduce, as a practical instance, that the last instrument erected at the Royal Observatory, Greenwich, and to which I attach great importance, was constructed by the mechanics of Ipswich; whilst I am at the present time in negotiation with one of the mechanical establishments in the town, for another instrument of considerable importance in astronomical observations. To this I may add, that the whole of Astronomy is geometrical in its character, and that a great part of it is mechanical. I mention these things to show that the alliance between astronomers and mechanics is very close indeed; and this being the fact, I shall endeavour to do for the mechanics the best in my power. What I offer on this occasion will be offered with hearty good will, and if the Lectures be not successful, I hope the failure will have arisen from no fault of my own

Perhaps I may be allowed to make another remark. I should wish to invite especially the attention of those who are commonly called working-men, to the few Lectures I propose to deliver. The subjects upon which I have to treat are commonly regarded as rather beyond their reach; I take this opportunity of saying that the subjects of the Lectures will not be beyond any working-man's comprehension. Everybody who has examined the history of persons con-

cerned in the various branches of science, has been enabled to learn that, whereas on the one hand those who are commonly called philosophers may be as narrow-minded as any other class, and as little informed; so on the other hand, those who have to gain their daily livelihood by handicraft, may associate their trades or businesses, whatever they may be, with accomplishments of the most perfect and the most elevated kind. I think, then, it is right I should repeat, that these Lectures will be directed in some measure with the object of being perfectly comprehended by that class of people. It is not my object, however, to deal with what may be called the picturesque in astronomy. I have proposed it to myself as a special object, to show what may be comprehended, by persons possessing common understandings and ordinary education, in the more elevated operations of astronomical science. The Lectures will be, therefore, of what I may call a mathematical kind. But in speaking of this, I beg that the ladies present will not be startled. I do not mean to use algebra or any other science, such as must be commonly of an unintelligible character to a mixed meeting. When I use the word *mathematical*, I mean that it will be my object to show how the measure of great things may be referred to the measure of smaller things; or to sum up in few words, it will be my object, in an intelligible way, to show the great leading steps of the process by which the distance of the sun and the stars is ascertained by a yard measure—the process by which the weight of the sun and the planets is measured by the pound weight avoirdupois. Occasionally I shall be prepared to go into details; but my principal business will be to show the great steps upon which those who wish to

study Astronomy may enter, and by which they may attain a general comprehension of the rules which will lead them from one step to another.

I shall now proceed with my subject.

We will consider what are the general phenomena of the motions of the stars which are to be observed on any fine night. I must observe in the first place, that I shall use the term *east* to denote the whole of the horizon extending from the north point, through the east point, to the south point; the term *west* to denote the whole of the horizon extending from the south point, through the west point, to the north. Now, if we look out on any fine night, the first general fact that we observe is this—by watching that eastern horizon from time to time, through the whole extent from north to south, we see stars are rising; and by watching that western horizon from time to time, through the whole extent from north to south, we see that stars are setting. By looking out at different times in the course of an evening, you will see these things as I have pointed out. The next general fact which you will observe is this—that the stars do not rise perpendicularly. They rise obliquely; and those which rise near to the south or near to the north rise very slantingly indeed. Those nearest to the east rise at a certain slope, which is different for every different place upon the earth. Those which set near to the north or near to the south set very slopingly; those which set nearest to the west set with a sharp incline. This is the case so far as regards merely the rising and setting of the stars. But if you trace the whole path of any one of these stars, it describes such a course as the following. It rises somewhere in the east, in the sloping direction I have described; it continues to rise with a

path becoming more and more horizontal, till it reaches a certain height in the south, where its course is exactly horizontal; and then it declines by similar degrees, and sets at a place in the west, just as far from the north point as the place where it rose in the east. If you select a star that has risen near to the north, it takes a long time in rising to its greatest height, it rises to a higher place in the south, and sets by the same degrees. Lastly, if you look to the north, and give your attention to those stars which are fairly above the horizon, you find the stars going round and describing a complete circle: these stars are called circumpolar.

Here I would remind my auditors that it is necessary, in order to understand a Lecture upon Astronomy, that they should have a little previous knowledge of the science—that they should know the names and situations of some of the more conspicuous stars, otherwise it will be impossible for them to proceed. I therefore assume that a portion of my audience possess this requisite knowledge. I presume you know which is the Polar Star; I presume also that you know which is the Great Bear. Now, these are objects of such importance, that nobody ought to think of entering an astronomical lecture-room who is not acquainted with them. There is another star remarkable for its brilliancy, which is in this country circumpolar, called Capella; and there is another star, which is also nearly circumpolar, it is the bright star in the constellation Lyra.

Now I will call your attention to each of these in succession. The Polar Star is one which, roughly speaking, does not change its place during the whole night. Whenever you look out you find it in the same place. But speaking a little more accurately,

it does change its place and move in a small circle. If you examine the stars of the Great Bear, you will observe that they turn in a circle considerably larger than that of the Polar Star, but they are still visible in the whole extent of the circle, and they turn completely round in it, without descending below the horizon. If you examine the next bright star Capella, which you will find on the globe in the constellation Auriga, you will find it describes a circle also, of which the Pole Star is apparently the centre. It goes very near the horizon when lowest in the north, and almost over our heads when lowest in the south. If you examine the movement of the last of the stars I have mentioned, namely, the bright star in the constellation Lyra, you will find it moves in such a circle that it as nearly as possible touches the horizon. In the south of England it just descends below the north horizon; here (at Ipswich) it does not, but it passes so near the horizon that it can rarely be seen in the north.

Thus, if we fix a straight rod in a certain standard direction, pointing nearly, but not exactly, to the Polar Star, we find that the stars which are close in the direction of this rod, as seen by viewing along it, describe a very small circle; the stars further from it describe a larger circle; others just touch the northern horizon; whilst, in regard to others, if they do describe a whole circle at all, part of that circle is below the horizon; they are seen to come up in the east, to pass the south, and to go down in the west, and they are lost below the horizon from that place till they rise again in the east. These are the fundamental phenomena of the stars. It is important that any person, who wishes to understand Astronomy, should look into the matter, and see with his

own eyes that the stars really do partake of these motions; that the Polar Star does nearly stand still; that the stars at various distances from the Polar Star, do move round in the way I say, one in a circle of one size, and another in a circle of another size: that others do move round in circles still larger, so that at their lowest points they just touch the north horizon; that others move round in circles so large that the lower part of these circles is lost, whilst the higher part rises above the horizon. It is of importance that anybody, who wishes to understand Astronomy thoroughly, should look out, and see for himself, that these things do happen in the way I have attempted to describe; by the observations so made, he will acquire a conviction of the truth far deeper and more lasting than from anything that can be pointed out in a course of Lectures.

From observing the way in which these motions take place, that we may assume one point of the sky as a centre, and that the movements of the stars are of such a nature that they will appear to turn round that one centre; the first idea that naturally occurs is, that the starry heavens, as we see them (I do not affix any precise meaning to that term), or a shell in which the stars seem to be fixed, do turn round an axis. It is necessary to show that this is supported by accurate means of observation. Now there is one instrument in use in the best Astronomical Observatories, which is specially intended for the elucidation of this phenomenon—it is the instrument called the Equatoreal. I should be glad if some of the wealthy manufacturers in this town would set up an Equatoreal instrument. The Equatoreal is an instrument, which, in one form, is represented in Figure 1. It turns round an axis

AB, and the axis is placed in that direction which

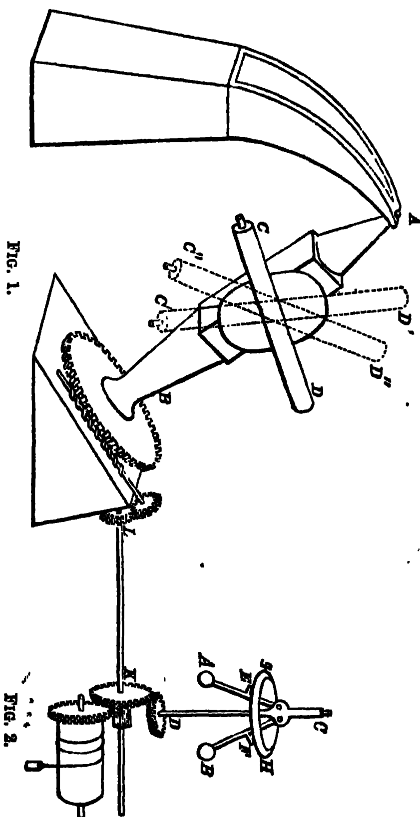

FIG. 1.

FIG. 2.

leads to the point of the sky around which the stars

appear to turn, and which is not far from the Polar Star. The axis being adjusted with great accuracy in that direction, the instrument turns round that axis, and it carries the telescope CD, which, of course, so long as you give it no other motion, retains the same inclination to that axis; but to which you may give another motion, so as to place it in different positions, as C′D′ or C″D″, directed to stars in different parts of the heavens. The instrument, then, is employed for the purpose of giving evidence as to the motions of the stars. It is used in this manner. The telescope is directed to any one star, and then by turning the instrument round the axis, it is found, that without any alteration in the position of the telescope in relation to the axis, the telescope will follow the star from its rising to its setting. And it is the same wherever the star may be, whether near the Pole, (in which case the telescope is in such a position as C′D′, very little inclined to the axis,) or far from the Pole, (in which case the telescope would be much inclined to the axis, as in the position of C″D″,) upon turning the instrument round its axis, the telescope still follows the star. This is a fact of accurate observation, for the confirmation of which this kind of astronomical instrument is peculiarly adapted. In this way it is established as a general fact, that all the stars move accurately in circles round one centre.

But there is another important thing to discover—with what rapidity do the stars turn? Do some travel quicker than others? Do some go quickly in one part, and slowly in another? Now, we have most accurate means of determining whether the speed be irregular or uniform, as regards the speed of any one star in any part of its motion—whether the

speed be irregular or uniform, in comparing the speed of one star with the speed of another star. I think that the best criterion which I can give is by a piece of mechanism which has been contrived, and applied to this purpose. (See Figure 2.) The best Equatoreals are furnished with a racked wheel attached to the axis, in which works an endless screw or worm, as at E, Figure 1. By turning it, the whole instrument is made to revolve. This worm, or screw, is turned by an apparatus which is constructed expressly for uniform movement. Various contrivances have been used for making this motion as uniform as possible. The one usually adopted, with some modifications (as represented in Figure 2), depends on the use of centrifugal balls AB, similar to those which are used to regulate the motions of steam engines. Everybody knows well that whirling these balls round by the rotation of the axis CD, to which they are attached, causes them to spread out. When the speed has reached a certain limit, the spreading out of these brings the moving parts, as at E and F, into contact with the fixed parts GH, and produces a degree of friction which prevents further acceleration; and thus a uniform speed is produced, with very great nicety. This contrivance is in constant use on my Equatoreal at the present time. You will observe it is essential to have a machine moving uniformly. In the motion of a common clock, though the movement from day to day, from hour to hour, and from minute to minute, is uniform, yet it is not so with the smaller divisions of seconds: the clock works with jerks, and does not go uniformly. Now the machine here is going on without any jerks—with a smoothness and uniformity scarcely to be obtained by any other apparatus. In all the best Observatories

in Greenwich, Berlin, Paris, and all others of any importance, this apparatus is used; in fact, it is used also in all the leading private Observatories, and the clock work which I now exhibit is borrowed from a private Observatory—from the Observatory of Dr. Lee. A spindle KL from this apparatus is attached to the worm which carries the Equatoreal. It makes the telescope of the Equatoreal revolve round the axis uniformly, and it thus gives us the means of ascertaining, with the utmost exactness, whether it be true, or whether it be not true, that all the stars do move with equal angular speed around one imaginary axis. When this machinery is in play, the telescope is adjusted, and pointed to the star. Whether it be turned to a star near the Pole, or to a star at a distance from the Pole, the effect is this—that the star is constantly seen in the field of view of the telescope—the telescope turns just as fast as the star moves.

Observe now the results obtained from these things. The first thing I mentioned was, if the telescope be directed to a star, and the instrument be turned, it follows the star in the whole of its course. The next result, which is particularly connected with the use of this machine, is, that the same uniform motion round the axis follows any of the stars, wherever you select them. This is the same as saying that the stars move, as it were, all in a piece; and when you come to examine how it bears on Astronomy, you cannot attach too great an importance to these results. It is indeed the great and fundamental principle of Astronomy, that the stars do move as if they were attached to a shell, or in other words, that they move all in a piece. As to the explanation of that, I shall not trouble you at present. I simply call attention

to the fact, that the stars move all in a piece—either, that they are connected with some one thing turning upon an axis, or that they stand still while the earth turns round an axis of its own; one or other of these things is certain.

Having now come to that result, as one which is generally established, I shall just mention a slight departure from it. Perhaps you may be surprised to hear me say the rule is established as true, and yet there is a departure from it. This is the way we go on in science, as in everything else; we have to make out that something is true; then we find out under certain circumstances that it is not quite true; and then we have to consider and find out how the departure can be explained. Now this is the fact. When we have a telescope of considerable power attached to the Equatoreal, so that we can see a small *departure from the centre of the telescope in the* position of the star we are looking at, and when we trace the course of that star down to the horizon, we find this as the universal fact—that though the instrument be set up as carefully as possible, yet the star is not quite so near the horizon as we are led to expect. What can the cause be? There is a consideration that explains it perfectly—it is what is called refraction.

In order to see what refraction is, we may advantageously examine refraction on a larger scale. In a room generally darkened, let a lamp be introduced, as at A, Figure 3, and let it shine through a hole B in a screen CD, so as to produce a spot of light E on the wall. Place in the course of that ray of light a trough F, whose sides are pieces of plate glass. Now pour some water into the trough, and see what effect it produces. You will observe that the light is

immediately thrown to the top of the wall, as at G. If the hole in the screen be so large that it is not

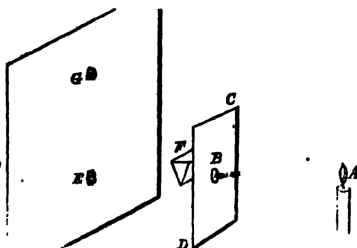


FIG. 3.

entirely covered by the trough, there will still remain a little light on the wall below, which shows the original direction. You will now see how much the direction of the light has been diverted by the action of the water in the trough. That effect is produced by the refraction of the water. It did not exist before the water was there, but it does exist now that the water is in the trough. I will now show the bearing of this matter on the subject of the disturbance in the position of stars. Figure 4 represents the prism of water we have been looking at.

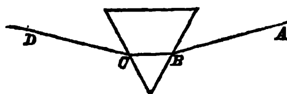


FIG. 4.

The effect of it is this—a beam of light, coming in the direction of the line AB, does not pursue its original direction, but when it is received by the

prism of water, it is turned in the direction CD. If you examine the prism, (as it is usually called in Optics, meaning the same form as that of a trough), you find that the point of it is downwards—the effect of it is that the beam of light which comes in this direction AB, is turned in the direction CD, or more upwards. There is a rule on this matter, which is thus expressed—that the course of the light is always turned to the thicker part of the prism. Or if you observe what is the bending of the light at the two surfaces of the prism, this is the way in which it may be expressed—when the light comes from the air into the water, its direction is bent more nearly towards the direction of the line which is perpendicular to the surface—when it goes from the water to the air, it is bent further from the perpendicular.* In the particular use of the prism, with its point downwards, these two things are combined in such a manner, that at each of these surfaces the direction of the beam of light is bent upwards. Of course you will infer that if the prism were turned in the oppo-

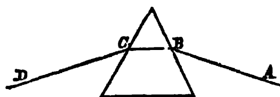


FIG. 5.

site way as at Figure 5, so that its point was upwards,

* The reader is particularly desired to remark that the word *perpendicular* does not mean *perpendicular to the horizon*, or *vertical*, unless it is so expressed. When the expression *perpendicular to the surface of the glass is used*, it means what a workman would probably call *square to the surface of the glass*. The *vertical* direction at any place is that of a plumb-line hanging there, or *perpendicular* to the surface of still water.

then the course of the light would be bent downwards.

Now, as regards astronomical observations, we have no water or glass concerned; but we have a thing which produces refraction, and that is atmospheric air. The common air produces refraction. The visible exhibition of this refraction is one of those nice experiments which I cannot attempt to exhibit to an audience like this. But it may be shown in various ways; as, for instance, by forming a prism of glass, and compressing more air into it; or again, by exhausting the air from it. It is shown that the effect of air is precisely the same in kind as the effect of water, though much less in degree. It may be stated as a general law, that where light enters from external space into air, or into water, or glass, or diamond, if you please, or any other transparent substance—where light enters from external space into any one of these substances, its course is bent in such a direction that it is more nearly perpendicular to the dividing surface than it was before. Now, having laid that down as a general law, let us see what its application will be to atmospheric air. In making astronomical observations, let

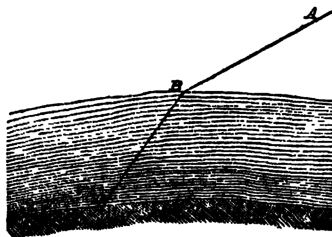
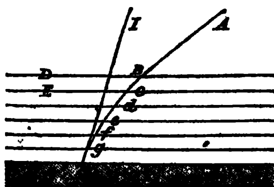


FIG. 6.

us assume that Figure 6 represents a part of the

earth, covered by atmosphere—the black part being the earth, the dusky part the atmosphere. Suppose a beam of light is coming in the direction AB from a star, and suppose that at B it comes on the atmosphere—it is coming then here exactly under the same circumstances in which in Figure 5 the beam of light comes upon the surface of the prism. According to the law which I have just mentioned, it will be bent in such a manner that its direction after it has entered the atmosphere is more nearly perpendicular to the bounding surface than before. Therefore, in conformity to that law, it is bent in the direction BC, and it reaches the eye of the observer at C, in the direction BC. If you observe the relation which the second line BC has to the first AB, you will see it is more nearly perpendicular to the horizon; or, standing at C on the surface of the earth, you have to look a little higher to see the star than if you were on the outside of the atmosphere, at B; or the star, in consequence of the action of the atmosphere, appears higher. Now this I have mentioned would be the case if the atmosphere had a definite boundary, and were uniform throughout its extent; but the same thing takes place if the atmosphere has not a definite boundary, and varies in density from stratum to stratum—the same effect takes place from one stratum of the atmosphere to the next.*



* Thus, if we suppose the atmosphere to consist of a series of parallel beds or strata, Bc, cd, de, &c. each of which is of uniform density throughout, the ray AB falling on the boundary BD of the uppermost stratum will be bent in the direction Bc, so as to be more nearly perpendicular to

In this manner we find there is a rational explanation of this too great elevation of the stars. 'Taking as foundation the established law of optics, determined by experiments on glass and water, and computing from this what ought to be the deflection of light, and what ought to be the elevation of the star produced by the refraction of light by the atmosphere, and applying that as a correction to the observations made by the Equatoreal Instrument, of which I have spoken, it is proved that the whole thing comes quite right—that the stars move exactly in circles, not approximately, but (as far as the human eye and instruments can discover) exactly as if they turned uniformly round one imaginary axis. This is the grand fact which must be regarded as the foundation of astronomy.

I shall now mention, in as few words as I can, how observations of all kinds are made, and how upon these observations the most accurate astronomical determinations are based. In the first place we will show the use of the telescope, and how it is used with wires in the field of view. The instrument thus fitted up is not used for mere gazing, but for accurate observation. If you go into an observatory, and look into any of the telescopes, you will see a set of bars. It will be perhaps beyond your comprehension what these bars are, and what they are for. Stars are seen to pass these

BD. Again, when it reaches cE, the boundary of the next stratum, it will in like manner be bent in the direction ed. The same thing will happen every time it comes to the boundary of a new stratum; and at last, when it reaches the earth's surface at C, its direction will be Cg. The star, instead of appearing to the observer at A, will consequently be seen at I, in the direction of Cg.

as if the stars and the bars were at the same distance from the eye. These bars are in reality fine cobweb threads, or something of the kind, fixed in the telescope very near to the eye. Perhaps Figure 7

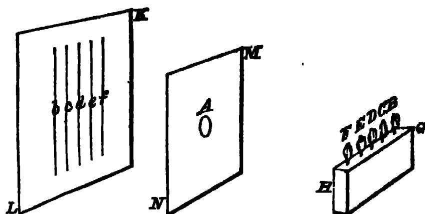


FIG. 7.

may serve to illustrate the construction of the telescope. There is no tube; but that is immaterial. At A is what we call a lens, that is to say, a piece of glass convex on both sides, and therefore thickest in the middle. It is here supposed to be fixed in a hole in a wooden screen MN. The property of this lens of glass is, if there be a luminous object in the distance, it collects all the light from that object; and instead of suffering it to go out in a broad sheet of light, it makes it contract so that the light from each point in the object is collected at a corresponding point on the screen; and therefore all the corresponding points of light on the screen, which belong to the original points of light in the original luminous appearance, when put together form an image which is exactly similar to the original object. The image, however, is turned upside down, because the light which comes from the upper part of the luminous object and goes through the lens, passes downwards towards the lower part of the screen KL.

These properties of a lens can easily be proved by experiments with a common burning glass, or a reading glass, or spectacle glass, such as is used by elderly people.

Suppose, now, that the stand GH is placed on the south side of A, and that a lamp is slid along it successively from B to C, D, E, and F. This movement exactly imitates the apparent movement of the stars as they pass across the south, travelling from the east to the west. The effect of it is, that if the screens are placed at proper distances, a spot of light is seen on the screen KL, moving in the opposite direction, as from *b*, successively to *c*, *d*, *e*, and *f*. Now, if there are traced upon KL a set of bars or dark wires, the spot of light passes over them in succession, first over one and then over another. Now this is truly and veritably an astronomical telescope. At A is the lens forming the image of the star—on KL is the set of wires in the field of view—if you placed an eye-glass on the other side of KL, and viewed the wires with it, you would have a complete astronomical telescope. This is the arrangement by which astronomical observations are really and truly made. Every astronomical telescope intended for accurate observations is fitted up with wires of this kind.

On looking to the south with the naked eye, the star travels from right to left. But on looking into the telescope with an eye-glass, as on the other side of KL, the image of the star is seen travelling from right to left; and its speed is so much magnified by the magnifying power of the telescope, that the motion is sensible and even rapid. It goes over the bars in succession, and one of the duties of the observer is to note the time at which it passes over every one of

these, and to take the mean or average of all, so as to diminish the error of a single observation. Having shown the way in which the transit of the star is observed over a series of bars, I proceed to point out the way in which it is made useful for the determination of some of the most important points in Astronomy.

First of all, I wish to point out what is the thing we want to do in representing the position of the stars, and what are the general principles of fixing that position. There is a term we use in mathematics—co-ordinates; it is a word not used in common language, and I would avoid it if possible; but it is necessary to use some word which will convey the idea; and its meaning will be perfectly intelligible if you consider how you are to represent the position of anything whatever. Suppose that we have before us a celestial globe, with stars and other objects upon it. How are we to define the positions of those? The thing to which I desire to call your attention is this—that where we have anything of a surface, real or imaginary, we must have *two* elements of some kind to define the position of any point upon it. In Figure 8, suppose that AB represents a wall; D a

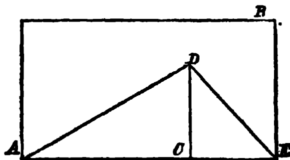


FIG. 8.

speck of dirt upon it. I want to define the position of that speck of dirt. What could I do? I could

measure the distance AC horizontally from one end of the wall, and CD vertically from the floor. That would define it accurately, and I could write down the measures in figures, so that a person at any distance could make a speck in a position exactly similar on another wall. I might do it in other ways. I might measure the distance AD from the corner A, and the distance ED from the corner E, and describing circles with these sweeps in my compasses from each corner in succession, I should be able to find exactly the position of that speck of dirt. I might do it in another way, too. I might say, if I go from the corner A to that speck of dirt D, the distance is so many feet, and the inclination of the line AD to the horizon is such an inclination as I can represent. That would do. But, in whatever way I do it, I must take *two* measures; there is no way in which it is possible, in the nature of things, that the position of that speck of dirt on the wall, or the position of a star in the sky, can be represented, except by *two* elements.

Now the question presents itself. What are the two elements most convenient for representing the position of a star in the heavens? There are two elements which, ever since accurate astronomical observations began, have been fixed on by all astronomers as the most advantageous. One is thus described: supposing we can fix on the imaginary pole or place of rotation of the stars, then one element is the distance of the star, as measured from that pole in degrees. I will speak in a short time of what is really meant by a degree. The other is, supposing the celestial globe, or the sphere of the heavens, to turn round an axis, as we have shown it does; then the question is, how far has it to turn from a certain

position before that star, whatever it is, comes under the meridian. If we can write down in figures, (for these are the things by which alone we can preserve a satisfactory record)—if we can write down in figures how far the globe has to turn from a certain position, till any one star comes under the meridian of the globe, or under the imaginary meridian which passes over our heads; and if at the same time we can tell how far the star is from this pole, round which the whole of the sphere turns, we can fix the place of the star. These are the two co-ordinates. I pray your attention to these things, which are necessary for determining the position of a star—one, how far the globe must turn before the star is on the meridian; the other, what is the measure of the distance from the pole of the heavens to the star at the time when it does come on the meridian, or, indeed, at any other time, as that distance does not sensibly change in a day.

The thing to which I would first direct your attention is, the way in which we ascertain how far the globe must turn before the star comes into the meridian. Figure 9 represents what is called the Transit Instrument. It is an instrument in perpetual use in every observatory. You see the instrument is not adapted to gaze at all points of the heavens; in ordinary use it can be turned round the axis AB, and has no other motion whatever. Now, what we want to do with this transit instrument is, to supply the place of the brass meridian of a common celestial globe. We cannot put a brass meridian over the heavens, or over our heads; but we want to make a telescope move in such a manner that the line CDE passing along the telescope, and prolonged to the starry heavens, shall exactly describe a curve resembling

the brass meridian. Consider, now, the various conditions necessary. First, every person who is

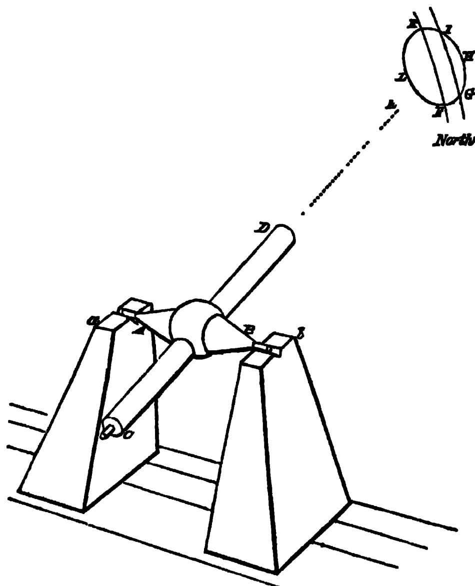


FIG. 9.

acquainted with the celestial globe, knows that the brass meridian ought to be perpendicular to the horizon; for securing that condition in the curve described by the transit instrument, the two points AB must be exactly level. In the next place, the brass meridian of a common globe is not what is called a small circle, but it divides the globe into two equal parts. For that purpose it is necessary that

the telescope CD should be square to its axis AB. The astronomer ascertains whether it be truly square or not, by looking at a distant mark, first with the pivots A,B, of the instrument resting on the piers *a*, *b*, and then with the axis turned over, so that the pivots A,B, rest on the piers *b*, *a*. If the telescope points equally well to mark in both positions of the axis, the telescope is truly square to its axis. The third condition is, that when the axis is level, and the telescope is square to its axis—on turning the instrument round its axis, the line CDE shall pass through the pole of motion of the celestial sphere. Now, the way of obtaining this condition is as follows:—We take advantage of that admirable Polar Star, which is a blessing to astronomers of the northern hemisphere. The Polar Star, as I have said, turns round like the rest, although in a small circle. Let FGHIKL represent the circle in the sky in which the Polar Star turns round in the order FGHIKLF. Suppose that in turning the transit instrument round its axis AB, the line CDE prolonged will trace on the sky the line GI or FK, as the case may be. The Polar Star in its revolution passes that line twice. Now, what we want is, that that line CDE, carried on to the sky, should be so directed, that in the motion of the telescope it should pass exactly through the centre of the circle which the Polar Star describes, and therefore that it should divide into two equal halves the circle which the Polar Star describes. We ascertain it in this manner. We can measure the description of the parts of the circle of the Polar Star by time. One of the most important parts of the apparatus by which that astronomical observation is made, is a clock. The clock should go well, and should beat loudly and distinctly. The astronomer

observes the Polar Star when it passes the transit instrument at its upper passage, as at K, and also when it passes its lower passage, as at F. If these are twelve hours apart, we know that the transit instrument is in its proper position. For as the star describes the whole circle in twenty-four hours, if the times of passing at F and K are twelve hours apart, there must be exactly half the circle between them, and therefore the line FK must pass through the centre of the circle, or through the Pole of the heavens. But supposing the transit instrument were a little out of position, so that the line, described by CDE prolonged, would be GI, then the star would require more than twelve hours to pass from its visible upper passage at I, through KLF, to its visible lower passage at G, and fewer than twelve hours to pass from its visible lower passage at G, through H, to its visible upper passage at I. In this manner we are enabled to adjust this transit instrument to its position with the utmost accuracy.

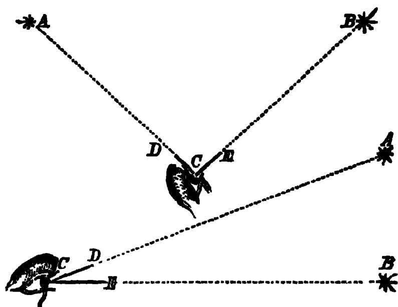
Having explained the manner in which the transit instrument is placed accurately in its proper position, I will now explain its use. I will assume you are looking to the south. The observer stations himself at his transit instrument, not looking at all parts of the sky, but waiting to observe the stars as they pass the meridian. The clock is going all the time. A star is seen to be approaching the meridian: the observer directs the telescope so as to observe the star when it actually crosses the meridian, and then looks into the telescope. In the telescope he sees the wires, and sees the image of the star travelling along, and he observes the passage of the star over every wire. Just before the star begins to pass, he looks to the clock face for the hours and minutes,

and he then listens to the clock, which beats seconds—in that manner he gets the hour, the minute, the second, and the fraction of a second, at which that bright star passes every wire, and by taking the mean or average of these, he finds the time at which the star passes the meridian. He looks again, and he sees a planet coming into the field of view. He directs his telescope to that planet, and in like manner he gets the time by the clock at which that planet passes the meridian—the hour, the minute, the second, and the fraction of a second. He sees another star. The telescope is moved to the proper position—he notes the time in the same manner, and he finds the hour, the minute, the second, and the fraction of a second, as before. Another star comes in the same way. Such is the duty which a transit-observer has to perform—the watching of objects which are passing the meridian in endless succession. He has this instrument, which is confined in its motions to the meridian, and which admits of no other motion; and the clock, by which he notes the hour, the minute, the second, and the fraction of a second; by the use of the various wires, he observes the time at which the object passes each wire; and by taking the mean of all, he finds very accurately the time at which the object passes the meridian: such are the duties of the transit-observer.

The next thing is to ascertain the elevation of the object when it passes the meridian. Now before we enter upon the use of the Mural Circle, I must offer a word or two upon Geometry. I dare say everybody here, like myself, has in his time, studied books containing measures—so many barleycorns make an inch, so many inches make a foot, so many feet make a yard, etc., as well as so many yards make a mile,

and so many miles make a degree. But the publication in a book of measures of such an expression as "69 miles make a degree" is in the highest degree reprehensible, as giving false ideas on one of the most important expressions in science. No schoolmaster ought to introduce books into his school, teaching that 69 miles make a degree. What do we mean by a degree? The use of the word degree is to define inclination, and it ought to be looked upon as defining a measure of inclination only, and not as defining a measure of length. If I had to describe the position of two arms of a pair of compasses, I should say they were inclined; but the notion of their inclination is entirely different from the notion of a measure of length. But we want some means for describing how much these two arms are inclined. Now the method of describing how much these two arms are inclined, is got at in this way: we use the word *degree* for a certain small inclination, such that if we first give one arm an inclination of one degree to the other, then incline it one degree further, then one degree in addition, and so on to 360 degrees, the arm will have gone through the whole circle of inclination, and will have returned back again to its first position. But these degrees, as you will perceive, have nothing to do with lineal measures; they are inclinations, and nothing else; they have nothing more to do with lineal measures than they have to do with pounds weight, or pounds sterling. We do, however, find it necessary to use the word degree in determining what might at first sight appear to be linear measures. For instance, if a star be seen at the point A, Figure 10, and if another star be seen at the point B, and if I want to measure the distance between them, I say they are so many degrees apart; but yet I do not

mean any number of miles, or any lineal measure. If I apply the hinge C of a pair of compasses to my



FIGS. 10 & 11.

eye, and direct one arm CD to the star A, and the other arm CE to the star B, and then if I observe the inclinations of these arms; if the arms are square, one arm has made one fourth of the complete turn round from the other; and as we call the whole circle 360 degrees, one fourth of the turn round when the compasses are square is 90 degrees, and we say that the stars are 90 degrees apart. If instead of that, I have to put the arms of the compasses in a less inclined position, as in Figure 11, the distance of the stars may be 50 degrees, or 30 degrees, or some smaller number of degrees. This must be fully understood before we can enter upon the explanation of the Mural Circle.

This Mural Circle is an instrument very much varied in form. Figure 12 represents the instruments in use at the Royal Observatory, Greenwich, at Edinburgh, and Cambridge. A is a stone pier which supports the axis of the instrument, and to which

microscopes a, b, c, d, e, f, are attached. The face of the pier which carries the microscopes, fronts either

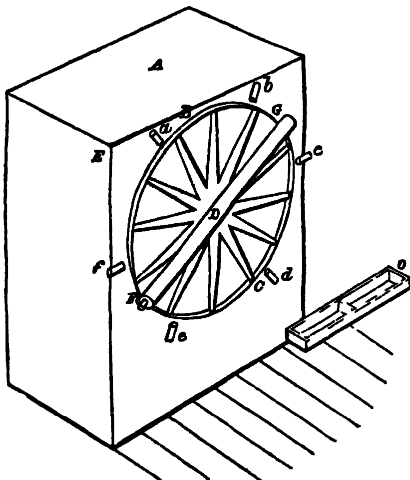


FIG. 12.

the east or the west. The construction of the instrument is this; there is a circle BC, which turns round an axis DE, (not visible in the view) that passes through the pier A. The circle BC has the telescope FG attached. This circle is graduated into degrees and minutes and other sub-divisions on its outside, containing 360 degrees in its whole circumference. Its position would be sufficiently observed for ordinary purposes if there were a pointer fixed to the pier at one part, but there are reasons (depending on the liability of the axis to be disturbed in its bearing, and on the tendency of the circle to bend under its own

weight) which make it desirable that there should be pointers at several parts of it. In the instruments at Cambridge and Greenwich, and other places, there are six pointers; they are not ordinary pointers, but microscopes, by means of which the spaces between the divisions can be sub-divided with greater accuracy than they could be by other means. Therefore you will perceive very easily, that by the use of these microscopes, viewing the circumference of this circle, it is possible to determine and register the position of this circle, (and consequently the position of the telescope which is fastened to it,) with very great accuracy indeed.

In all measures, however, we want a starting-point. What we want to ascertain with the circle is, how far the telescope is pointed above the horizon. It is therefore a very important thing to ascertain what is the reading of the circle when the telescope points horizontally. There is a contrivance used in most modern Observatories for this purpose which is worthy of attention—it is the use of observations by reflection. Suppose that a star is seen by the observer to be approaching the meridian, he places a trough of quicksilver O in such a direction that the star can be seen by reflection in the quicksilver. When the telescope is pointed towards the reflection in the quicksilver, then we know that the telescope is pointed below the horizon, just as much as it is pointed above the horizon to see the star by direct vision. This results from the optical law of reflection. For in Figure 13, if FG be the position of the telescope placed to receive the light which comes from the star in the direction SG, and if F′G′ be the position of the telescope placed to receive the light which comes from the star to the quicksilver in the direction

S′O, and is thus reflected in the direction OG′, then by the law of reflection, S′O and G′O make

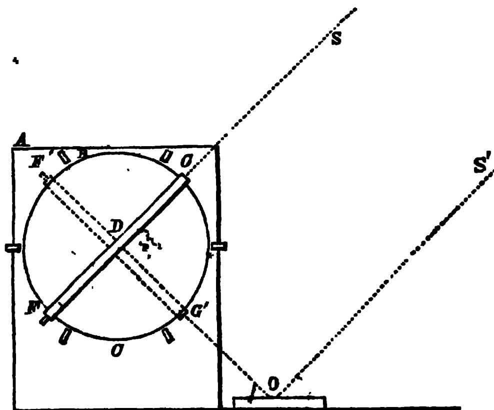


FIG. 13.

equal angles with the surface of the quicksilver. But as the quicksilver is perfectly fluid, its surface is exactly horizontal. So that G′O and S′O make equal angles with the horizon; and therefore FG′O points as much below the horizon as OS′ or FGS points above it. The observer therefore looks at the star by reflection in the quicksilver; he takes the reading of the microscopes; he then turns the instrument so as to see the star by direct vision in the telescope, and then he takes the reading of the microscopes; then by taking the mean between the reading of the circle corresponding to these two observations, it is certain that we have got the reading corresponding to the horizontal position of the telescope. That gives us a starting-point; and having got that, whenever we

observe a star in any position whatever on the meridian, inasmuch as we have got the reading of the circle when the telescope is directed to that star, and as we know the reading of the circle which corresponds to the horizontal position of the telescope—then by taking the difference between these readings, we know in degrees and minutes the inclination of the telescope, or the degrees and minutes by which the star is elevated above the horizon. The method of observation which I have described is going on at an Observatory every day. It is necessary, however, to remark that (as has been already said) every star appears too high, in consequence of refraction; a correction must therefore be subtracted from the elevation thus found, in order to discover at what elevation it would have been seen, if there had been no atmosphere about us.

Now, suppose that we observe the Polar Star. This star, though very near the Pole, describes a small circle round the Pole, and therefore goes as much above the Pole at one time when it is highest, as it does below the Pole at another time when it is lowest. Therefore, by taking the angular elevation above the horizon, in degrees, minutes, and seconds, of the Polar star when at the highest point above the Pole, and applying the proper correction for refraction; and taking its angular elevation in degrees, minutes, and seconds, when at the lowest point below the Pole, and applying the proper correction to this for refraction; and taking the mean between the two elevations so corrected; we get the true angular elevation of the celestial Pole. In that manner we have got the accurate calculation of the angular elevation of that Pole in the North side, round which the heavens appear to turn.

Now, allow me to point out what we have obtained with regard to these celestial objects.

By the use of the transit instrument, when properly adjusted, and the clock, we observe the time of transit of a principal star, and we observe the time of transit of any other objects, smaller stars, planets, or whatever else they may be. By means of these observations, we have a difference of times of transit. We can place no reliance upon the clock, except this, that it gives us the difference of time between the passage of principal stars, and that of other objects. Suppose that our clock is so adjusted, that if we observe the time of that principal star passing the instrument to-day, and again observe the time at which it passes the instrument to-morrow, the clock describes accurately twenty-four hours. If it does not describe accurately twenty-four hours, we know how great its error is in twenty-four hours, and we can apply a proportionate part of the error to every interval of time; so that it is in every respect as serviceable as if it were accurately adjusted. Supposing, then, that our clock was adjusted in such a manner that it indicated twenty-four hours, from the time of a principal star passing to the time at which the same object passed again—this amounts to saying, that it indicates twenty-four hours in the time in which the whole heavens turn round. Assuming, then, that the planet which we have observed passes the telescope one hour after the principal star passes, then we must conclude that the heavens have turned for one hour; or have performed one twenty-fourth part of their whole revolution, before that part of the heavens in which the planet is seen passes our meridian. And this is precisely one of the co-ordinates which, as I said, serves to determine the position of

the stars, or the planets, in reference one to another. What we want to know is, the interval of the successive times at which they pass the meridian. Assuming that the starry heaven turns uniformly, this interval (which in the instance above we have supposed to be one hour,) enables us, if we wish, for instance, to register the planet's place on a globe, to turn the globe one hour, or one twenty-fourth part of a revolution, from the position in which the principal star was under the meridian, and then we know that the planet which we have observed will be somewhere under the meridian, in that new position of the globe. That is the result of the observations with the transit instrument.

The next thing is, by means of the observation of the Polar Star with the Mural Circle, and by determining how high any other object appears when it passes the meridian, to determine the angular distance of any object from the Pole. These two observations amount to this:—the first gives the angular distance of the Pole from the north horizon. It is, however, rather more convenient to refer the position of the Pole to the point which is exactly upwards—usually called the Zenith. The change is very easily made; for as the angular distance from the Zenith to the horizon is ninety degrees,* we have only to subtract the elevation of the Pole (or of any other object) from ninety degrees, in order to obtain its zenithal

* The reader will easily understand this, if he remarks that upon opening a pair of compasses so that one leg points exactly upwards and the other leg points to the horizon; the two legs are then exactly square to each other, and therefore one leg has been turned away from the other by one-fourth part of the movement which would bring it round to the other again, or by one-fourth part of 360 degrees.

distance on the north side of the Zenith. Thus we find that between the Zenith and the Pole there are so many degrees, and minutes, and seconds, of angular distance. That is obtained from the observations with the Mural Circle, directed to the Polar Star. By using the same instrument in the same manner, but directed to a planet or other object, we find the angular distance from the Zenith to the planet on the south side of the Zenith. We have, then, got these two things: we have got the angular distance of the Polar Star from the Zenith on one side, and the angular distance of the planet from the Zenith on the other side. By adding these together, we have the angular distance of the planet from the North Pole. This is the other co-ordinate necessary to define the planet's place.

As I said before, by the transit instrument we have found what is the proportion of a revolution through which the celestial globe must be turned in respect to a certain fixed star, in order that we may fix the position of the globe when the body passes the meridian; and by the observations with the Mural Circle, we have fixed the distance of the object from the Pole, when that object passes the meridian. These are the two co-ordinates which completely define the planet's place. If we had a globe we could mark down the place of the object at once. Or, instead of this, the result of the two kinds of observation may be registered in figures.

I have referred the times of transit of planets and small stars to one principal star, supposing it taken as a point of departure. This method was adopted by my predecessor, Dr. Maskelyne, and by several of the best astronomers. Dr. Maskelyne adopted the bright star of Aquila, as his fundamental star; others,

however, have used several bright stars, whose relative positions have been well ascertained; and this is now the more usual course.

The methods of which I have spoken, give us the means of recording, with the greatest accuracy, the position of any object as viewed from any point of the earth, and we come to the same conclusions, as to the relative positions of the stars, wherever we may be placed. Thus, at the Cape of Good Hope, where there is an Observatory in the highest order, the relative positions of the stars are seen to be precisely the same as when they are viewed from the European Observatories. If you observe how many hours, minutes, and seconds, one star is before the other when it passes the meridian, and how many degrees, minutes, and seconds, one star is higher than the other when it passes the meridian—whether it is observed here or at the Cape of Good Hope, it amounts to precisely the same thing—there is not the slightest difference. From this we must draw one of two conclusions: either that the stars are, as it were, stuck in a shell of a very great size; or else, that the distance between the North of Europe and the Cape of Good Hope is unmeasurably small, compared with the distances of the stars; or that the distance of the stars is unmeasurably great as compared with the distance from the North of Europe to the Cape of Good Hope.

I shall now conclude this lecture. In the next lecture I shall treat of the figure and dimensions and rotation of the earth; of the movements of the Sun amongst the stars; and of the motions of the planets.

LECTURE II.

Recapitulation of Lecture I.—Investigation of the form and dimensions of the Earth.—Proof that the Earth *really* revolves.—*Apparent* motion of the Sun among the Stars, or *real* motion of the Earth round the Sun.—Permanence of an axis of Rotation.

IN the last lecture I endeavoured to point out to you the principal phenomena of the motions of the stars as observed on any fine night. And I called your attention to the fact, that these motions are performed in such a way, as to give us the idea of rotation round an axis inclined to the horizon; that some of the stars move very little; that others describe larger circles; that others just touch the horizon and descend below it; that others descend on one side and rise on the other side. I mentioned the names of two or three stars admitting of easy observation, as I am desirous that you should observe a little for yourselves, because you will acquire more knowledge from personal observation than from any lectures. The first of these is the Polar Star, which everybody ought to know; the second is the constellation of the Great Bear, which most people know as Charles' Wain; the third is the bright star Capella; the fourth is the bright star of Lyra. I described their motions: and I then pointed out to you that the observations were rendered more accurate by means of the instrument named the Equatoreal, which makes a telescope turn on an axis parallel to

the direction of the axis round which the stars appear to turn; and that we find, by fixing the telescope to the axis in such a position that it is directed to any one star, and then, by continuing to turn the instrument upon its axis, the telescope will follow the star from its rising to its setting. This I mentioned as establishing an important point, that the stars undoubtedly do appear to revolve round that axis. I then described the use of the clock-work for causing the Equatoreal instrument to revolve uniformly. And I pointed out to you, as a thing of importance, that when the clock-work is in action, to whatever star we may direct the telescope, however far that star may be from the Pole, or however near it may be to the Pole, the telescope does continue to revolve after it, so that the star is always kept in sight, or in the field of view. Inasmuch, therefore, as all the stars appear to revolve uniformly round one axis, it follows that the stars keep their relative places or positions, that is to say, the heavens turn as it were all of a piece. Of course there is no explanation of that, except one of these two—either that the heavens are solid and go all of a piece; or that the heavens may be assumed to be fixed or immovable, and that we and the earth are turning instead of them.

I then particularly mentioned that, taking advantage of this circumstance, instruments are contrived for daily use in every Observatory in the world, as adapted to defining the places of celestial objects. In the first place, I directed your attention to the transit instrument, as one of the most important instruments used in taking our observations. This is mounted like a cannon, turning upon two pivots, and possessing no other motion; these pivots resting

on stone piers, if the instrument is of a large size, or upon metal piers, if the instrument is of a smaller size; the telescope so adjusted, and turning in this manner, moves only in the meridian. And here it is important to remark that in all standard observations in Astronomy, the instrument is not turned to stars in any part of the heavens, but we have to wait until the stars come upon the meridian. We must so manage as not to be too late, or we lose our observation. The transit instrument must be adjusted in reference to our notion of what we want to observe. The object of all this is to define the places of the stars, in relation one to another; the places of the planets, the sun, the comets, the moon, in relation to the stars, and so on: in fact the use of all observing instruments of this class is to define the place of one object in relation to some one or other fixed objects. I then endeavoured to explain that, for exact definition of the place of an object, it is necessary to use a system of what, in mathematics, are called co-ordinates; and that, when the object is or appears to be upon a surface, two co-ordinates are necessary. By this term, I mean two measures of some kind or other; as distances from two fixed lines, or distances from two fixed points, or length of the line from one point and inclination of that line to the horizon. Thus, for determining the position of the stars in reference one to another, it is a matter of importance to choose the most convenient co-ordinates. Considering the stars as they are represented on the celestial globe, if we wish to define the place of any star, the most convenient co-ordinates we can use are these: in the first place, to see how far the globe must have turned from a certain position before the star passes under the brass meridian; and in the next place to see,

when that star passes under that meridian, how far it is from the Pole round which the globe turns.

I then pointed out that the transit instrument is one of the instruments particularly adapted to this purpose. The transit instrument does, by its motion on the axis I described, trace on the sky a curve exactly similar to the brass meridian of the globe, provided these conditions be observed: first the axis must be horizontal; secondly, the telescope must be square to its axis; and thirdly, when the telescope is turned to the north, it must in its sweep pass over the centre of rotation of the stars. All this I fully explained, but I give this recapitulation that it may be kept in recollection as we proceed with our lectures. By means then of this transit instrument, the condition of representing the brazen meridian by an imaginary track of the telescope through the heavens is fulfilled. I then mentioned that we make use of a clock in all our observations; that the way of using it is, having noted the time when some star or object passes the meridian, to find by the clock the interval of time until other stars or planets pass the meridian.

I may now add one subject which I omitted, and it is to state what we mean by a Sidereal Day. We observe on this day a bright star, for instance Arcturus, passing the meridian. We note the time by our clock, in hours, minutes, and seconds, and the fraction of a second. To-morrow we again observe the passage of Arcturus across the meridian. The interval between these passages is a sidereal day. A sidereal day is not quite the same as a common day. But I do not insist on that at present, because it is connected with other things, one of which is the motion of the sun. It is important to understand

that that is what we mean by a sidereal day. I cannot tell you now what sidereal time is, and for this plain reason: I have not yet got the starting-point which marks the beginning of the sidereal day. All that I can at present say is, that the interval from the time of the passage of one star one day to the time of the passage of the same star the next day is understood to be twenty-four hours of sidereal time.

Having proceeded so far in relation to the times of the passing of the stars, and the quantity of rotation which the globe must perform from the meridional passage of a fixed star which we know, to that of a planet or similar object whose position we want to determine, I mentioned the use of the Mural Circle, by which we determine the altitude of the object when it is passing the meridian. And here I must observe, that one of the most important adjustments of the Mural Circle depends on reflection from the surface of quicksilver. It is not my province now to allude to optics as a science; I merely allude to it to indicate a thing important to our present purpose: the law of reflection of light from a surface of quicksilver. The surface of the quicksilver takes a position parallel to the horizon, with a degree of suddenness and certainty to which we know nothing similar. Light is reflected from the surface of the quicksilver, just as it would be from a looking glass. Now, the thing which I wished to point out as the great practical fact is this: that supposing SG and S'O, in Figure 13, to represent the direction of the light coming from the star, and OG'F' the direction of the light reflected from the quicksilver; then the inclination of S'O or SG to the horizon is the same as the inclination of OG' to the horizon; and if S'O or SG approach nearly to a flat with the horizon, OG' will

approach nearly to a flat with the horizon; and if S′O or SG approach nearly to a perpendicular to the horizon, OG′ will approach nearly to a perpendicular to the horizon. These are the facts upon which the use of observations by reflection is founded. If we place this small trough O in such a position that the telescope looks into it, and if we see a star, we know that the light which comes from that star is reflected in such a manner that the position of that telescope must be as much inclined downwards as the position of the direct ray of light from the star is inclined upwards. From these observations we infer the position of the telescope when it is horizontal. I then pointed out to you that by the use of this, we ascertain the elevation of the Polar Star at its highest and its lowest positions, and that by taking the mean of these we have the height of the Pole; and therefore, getting the elevation of the Pole on one side of the Zenith, and getting the elevation of any other star or any object whatever, passing either on the same side of the Zenith or on the other side; by means of these we ascertain the angular distance of any object from the Pole.

Incidentally I had occasion to point out to you some other things. One of these was the use of the wires in the telescope. And I constructed, as it were, a telescope, Figure 7, though it has no tube. The telescope I have here made consists of a lens, representing the object glass of a telescope, and a screen, representing the field of view. Formerly telescopes were made without any tubes at all, so that the tube of a telescope is totally unessential. The construction I have got there is a proper representation of a telescope, forming an image of the object on the screen, which screen is in the place in which you

would see the object. Instead of looking at the screen from a distance, you may come close to it, and view it with an eye-glass on the side opposite to the object glass, and then the resemblance to the telescope is complete. You see upon it bars representing wires in the field of view. The object of these is to give definiteness and distinctness to the observations. Supposing I use the telescope by directing it to a star: if I see a star somewhere in the telescope, this is a very loose observation, because I have not sufficiently defined the place; but if I have wires in the telescope, and observe the star on any one of these wires, then I have observed it in a definite part of the field of view. The accuracy gained by that observation is very great indeed; it is the most important adjunct connected with the use of the telescope. Every surveyor knows the value of the wires in his theodolite telescope.

There was another thing I pointed out, which was that the rotation of the stars, when it is examined closely, is not so accurate as might be supposed at first, for this reason: that we always find that the stars near the horizon appear higher than they really are in fact—whether east, north, or west, they are always a little too high—I ascribed that to refraction. And I pointed out, as a law of refraction, by reference to a glass trough or prism of water, that if the light falls on a surface of glass, it is bent there in such a manner as to go more square to the surface. I had occasion in the last lecture to mention strongly my disapproval of the use of some words in a wrong sense. I shall now mention another word which is often used in a wrong sense: I allude to the word "perpendicular." Many people think that the word "perpendicular" means the position of a plumb-line.

It means no such thing. The proper word for describing that position is "vertical." Perpendicular is a relative word, and it ought not to be used without reference to something else. Vertical is an absolute word. Thus, in speaking of the word perpendicular, with reference to that trough of water or prism in the experimental case before us, I mean a line perpendicular, not to the horizon, but to the surface of the trough. I explain that particularly, because in connection with these matters, from defective education and other causes, false meanings are often given to words. The law of refraction is this: that when that beam of light represented by AB, in Figures 4 and 5, enters the side of the prism, it is bent into a position approaching more nearly to the direction of the line perpendicular to the surface. The refraction of light produced by glass or water is well understood. We know by experiments too, that air produces refraction. We apply the same laws which relate to water or glass, to the computation of refraction by atmospheric air. We find that air will alter the course of the light in such a manner that the beams of light enter our eye more vertically than they otherwise would do; that all objects will necessarily appear to be higher than they are in reality; and applying then a proper correction by the law of refraction, based upon experiments with water or glass, we find everything properly adjusted, and that the stars revolve round one axis most accurately in the manner we have represented. These, with some additions, were the main points of the subject of yesterday's lecture; I will now proceed to the lecture of to-day.

I have stated my intention of explaining how we measure the distance of the sun, and the moon, and

the stars from the earth by a yard measure. When I say that we really measure these distances by a yard measure, I do not wish to weary you by the word, and I do not wish to introduce anything inelegant; but I do wish to produce distinct and definite ideas in your minds; to urge this, that we really do make use of a yard measure, or something equivalent to it, as our fundamental measure for these purposes. I will now proceed to explain the first steps towards taking these measures.

The object we have in view is to measure a great distance upon the earth; a distance, for instance, extending the length of a kingdom. Figure 14 (see Frontispiece) represents nearly the whole of the British Islands. I wish to point out how the distance is measured from the Isle of Wight at A, to the Shetland Isles at B. In the first place I must tell you, that the distance has been measured with such accuracy that I think it likely that the distance is known with no greater error than perhaps the length of this room. Now, measures of this kind are effected by a system of triangulation. This is in some degree or other well known to every surveyor, but still I esteem it so important to the whole subject before me, that I shall point out to you the way in which it is done. Suppose, then, that we have three places, EFG, Figure 15; the two nearest, E and F, on a plain

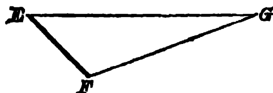


FIG. 15.

with even ground between them, and perhaps six or

eight miles apart; a third, G, at a considerable distance, perhaps inaccessible, at least in a straight line from E and F. I can measure the distance between E and F, because there is even ground between them. But how do we get the distance of G? In the first place we actually measure the distance between the two nearest, E and F. In the prosecution of surveys of this kind, it is a great object that we should choose ground favourable for taking the measure; it is necessary that the ground should be very level, and, if possible, firm. The line so measured is called the Base Line. Bases have been measured in the British survey on Hounslow Heath, Romney Marsh, Misterton Carr, Salisbury Plain, and other places; but the principal basis measured in the United Kingdom for several years past, and on which the measure of almost every part of the kingdom depends, is one in Ireland, traced along the east side of Loch Foyle, near Londonderry. It was measured on the sand; and the smoothness and level of this soil served well for the purpose.

Now this base is measured by a very troublesome operation indeed. You may think it easy to measure a straight line, but, in fact, there is nothing so difficult. In the first place, what are you to measure it by? Are you to use bars of metal? They expand by heat. It is to be measured by the yard. If so, what do you mean by a yard? By the measure of a yard we mean a certain distance, not something imaginary or variable, but a distance definite and certain. But we do not mean the length of any piece of metal, because it changes its state by the action of temperature: it becomes longer when hot, and shorter when cold. If I use a piece of metal, I say a yard means the length of this bar of iron or

brass at a certain temperature. Now, many bases have been measured with bars or chains of iron or brass, but in every part of the operation every possible care has been used to screen them from changes of temperature, by covering them with tents; putting perhaps half-a-dozen bars at a time in a row, with twenty yards of tent over them, so as to protect them effectually from the sun and wind. Having taken these precautions to guard them from the effects of changes of temperature, thermometers are placed by the side of the bars. Then by carefully observing the state of the thermometer, and knowing the expansion of the bars by heat, or their contraction by cold, we can ascertain what length these bars represent under the circumstances under which they are used. But there was another contrivance used specially in the Loch Foyle base. It was used for the first time there; it has been since used in India, and at the Cape of Good Hope. Figure 16 represents



FIG. 16.

a combination of two bars; one, ABC, of brass, and the other, DEF, of iron, connected at the middle BE, and having projecting tongues, ADG and CFH, which are connected with both bars at the two ends of the bars. Now, the use of the combination is this: brass expands by increase of warmth considerably more than iron, in the proportion of 5 to 3, as nearly as possible. If I arrange the bars in this manner, and choose points G and H in such positions that

DG is three-fifths of AG, and FH is three-fifths of CH; then the distance between H and G is not disturbed by the expansion of the two bars. The iron bar expands, the brass bar expands more; and by that increased expansion of the brass bar, the two points G and H are brought inwards by exactly the proper quantity. In this manner a means of measuring has been attained, which, in the judgment of many persons who have used it, is better suited to the purpose than anything else that has been used or adopted. I have described in detail this apparatus to show the extreme caution necessary in these matters.

A succession of combined bars like these are placed one after another, with a small interval between each and its successor; and then the question is, how is the interval between them to be measured? It will not do to make one bar touch the other, because expansions may be going on in one of the series of bars, and it would jostle the others throughout the whole extent. In the measure of which I spoke, this small distance was measured by means of microscopes; and these microscopes were so mounted (on the same principle as the bars) that the measure which they gave was not affected by temperature. In some of the surveys on the Continent, glass wedges have been dropped between the successive bars; in some others there have been sliding tongues used; indeed an infinity of contrivances have been used to overcome the difficulty. The effect of all this has been, that a distance of 8 or 10 miles has been measured to within a very small fraction of an inch. This is the first application of the yard measure, by which the distances of the sun, the moon, and the stars are to be measured. In figure 17,* EF represents the base

* See lithographic plate fronting title page.

on Misterton Carr, connected with the triangulation, by which the distance from A (Shanklin Down, in the Isle of Wight) to D, (a place called Clifton, in Yorkshire,) was measured.

The next thing to be done, having measured the length of the line EF, Figure 15, is to measure the distance of the signal G. It is, perhaps, on a mountain, perhaps with sea between it and EF. The object is to get the signal as far off as it can be seen. These signals have been observed at the distance of 110 miles. Signals in Ireland, on the Wicklow Mountains, and on Slieve Donard, have been observed from Ben Lomond, in Scotland; from Precelly and Snowdon, in Wales; and from Scaw Fell, in Cumberland. Having, then, measured EF, I wish to ascertain the distance of G. For that purpose I take away the signal at E, and plant a theodolite in its place. The theodolite is adjusted on the point E with the utmost care. Now, by means of this theodolite, making use of it in the usual manner, first of all I observe the signal F at the end of the base, and then turning it until I observe the signal G on the distant hill, I obtain the angle of an imaginary triangle GEF, if you may so call it. The triangle is, in fact, formed by the rays of light which come from the signal at one station, to the eye, or instrument, at the other; when I turn the telescope of the theodolite at E towards F, it is in the direction of one side, EF, of the triangle; and when I turn it so as to view the distant signal G, it is in the position of the other side of the triangle; and therefore the angle, by which the theodolite turns, is the measure of that angle FEG of the triangle. I then plant the theodolite at F; I direct it in like manner to the end of the base E, and then the light it receives is in

the direction of the side of the triangle FE; I turn it then to the same distant signal G. Therefore, by these observations, I have really and truly got, by the theodolite, the measure of the two angles of the triangle at E and F. Now, that is sufficient. Every person who has a knowledge of trigonometry, knows that if we have got the measures of the side EF, and of these two angles, we are able, either to construct the triangle on paper, or to determine, by calculation, the whole of its parts. Or, without pretending to understand or to have heard of such a word as trigonometry, any person can see, that by observing how much I turn the telescope at E, for instance, I can make the same turn of a line on paper; that I can make the two directions of the line incline to each other by that angle. Knowing how much this telescope has been turned from one object to the other, I can make the same angle on paper here; I can do the same for the other end of the base, and then, prolonging these lines until they meet, I get the distance of the distant signal. This is sufficient. But, to make assurance doubly sure, it is usual to place a third theodolite at G, and then to observe the signals at E and F. And the reason is this: we know by geometry that if we take the measures of the three angles at A, at B, and at C, in degrees, minutes, and seconds, and add them together, the sum will be 180 degrees; so that the observation of the angle at G is a verification of the measures of the two angles at E and F.

Now, then, we have made the first step in triangulation. Having measured the base line EF, by means of a yard measure, as represented by some of our standard rods, and having measured the angles,

we have, by the process I have described, got the length of these other sides of the triangle. Then, in like manner, having thus got the length of the side FG, we can use it as a base measure to determine the distance of the signal on another distant point; and thus we go on, step by step, until we get from one end of the kingdom to the other. I have represented, in Figure 17, the triangulation connecting Shanklin Down with Clifton Beacon. This is a part of the great Meridional Arc of England. Some persons, I have no doubt, are present now who have seen a place in the Isle of Wight, called Shanklin. North-east of the village is a high swelling Down; a point on this Down is the southern extremity of this triangulation. The names of the stations which follow are marked in the diagram. Signals were placed upon the successive stations; at each of these theodolites were placed; from each of these the signals were observed upon the other stations; and so we step on from one point to another, till we arrive at Clifton, a village in the south of Yorkshire; and (by continuing the triangulation) at Balta, in the Shetland Islands. The outlines of the counties through which the survey was made are roughly drawn on the diagram, in order to give a notion of the size of the triangles. The line, for instance, which connects Brill with Stow goes over the widest part of Oxfordshire, from a signal external to it on the eastern side, to another signal external to it on the opposite or western side. In this way we step over a country at few steps; and when the angles of all the triangles are accurately measured, the results may be laid down on paper. I have spoken of beginning from the south end of the triangulation; but in the process of

making the computation, we must (in this instance) begin from the north end, because it happens that the base EF is there.

The next thing is to get the direction of one of these lines. This is got by a transit instrument, or something equivalent to it, adjusted in the same manner which I described yesterday, by the Polar Star. The transit instrument suppose at K, Figure 17, is adjusted to the north, so that the telescope passes through the Pole: the telescope is turned down to the horizon, and a mark L is fixed by means of it in the true north direction on the horizon. Then the theodolite is used to measure the angle LKM between this mark and some other signal: and knowing the northern direction in that way, we are enabled to lay down the whole of the triangulation on paper, and to see how many yards the point D is north of the point A. That is the first result of the meridional triangulation through a country; it is a point which it is most important for us to understand by way of beginning.

Now, let us see how this is to be used. What do we want to ascertain? We want to ascertain something about the size and form of the earth. I remember a man in my youth who used to say he should like to go to the edge of the earth and look over. I don't think that many people, who have ever considered carefully the state of things around them are impressed with notions of that kind; but my friend was in his inquiries an ingenious man, a sort of philosopher in his way. Still, if he had looked about him, he would have seen that the earth did not present such a condition as would enable him to go to the edge of it and look over. I dare say that there are many persons here who have come by

sea from London to Ipswich, and have observed Walton Tower rising out of the edge of the waters. Many persons, too, who have gone across the Irish Channel, have seen the mountains on one side disappear, as if they dipped into the sea, and they have also seen the mountains arise out of the sea on the other side, perfect in shape, coming out by degrees, just as if seen rising over the brow of a hill. The inference is, that the water is curved, to produce these phenomena. These are to be seen in the course of ordinary expeditions; but those who voyage further, those who have gone to the Cape of Good Hope, know that, as they go on, every night they lose sight of our stars by degrees, and other stars come up on the other side. In a southern latitude they lose the northern stars, and they get more of the southern stars. All this leads us to the conclusion that the earth is something curved. Again, people have sailed round the earth. This was done for the first time by Magellan and his successors in command; and for the second time by Sir Francis Drake. From the time of Sir Francis Drake this has been done every year; ships are indeed almost daily prosecuting such voyages. It is a common thing for ships to sail in an easterly direction to Australia, and to return by continuing their eastward course, and not by coming back the same way they set out. The earth, therefore, roughly speaking, is something round, and there are limits to its extent.

Now, the question is, what is its extent? Having got a measure of considerable length by such a process as I have described, how can we use that to determine what is the size of the earth. In order to explain this, suppose Figure 18 to represent a slice of the earth, curved as a slice of the earth would be.

Now, you will understand from the description which I have given, that, in the first place, by measuring a base by means of a yard measure; in the next, by measuring successive triangles originating with that base; and therefore, in fact, computing the length of every side of these triangles by means of a yard measure; you will understand we have really ascertained, by means of the yard measure, the distance from the Isle of Wight A to the little Island of Balta B, in the Shetland Isles. Now, we want to measure the corresponding curvature of the earth, that is, to find how much the line drawn from A to B on the surface of the earth is bent. For that purpose we use an instrument called the Zenith Sector, Figure 19—a telescope swinging upon pivots AB, and having attached to it an arc CDE graduated into degrees and minutes. There is a plumb-line EF connected with the upper end of the telescope, or with one of the pivots; it is a very fine silver wire, supporting a weight F, which weight is hanging in water, to keep it steady. It gives us the direction of the

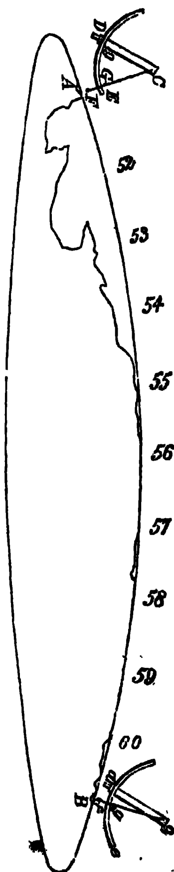


FIG. 18.

vertical there, or the direction of the perpendicular to the horizon, (see page 15.) Now, assuming that we are observing a star G very nearly overhead—it is plain if the telescope be directed to the star, then by observing the point of this divided arc CDE, which is crossed by the plumb-line, I have got a measure in degrees, minutes, and seconds, of how far the star is from the vertical. And the peculiar advantages of using this instrument, instead of the Mural Circle, are these: first, it is easier to carry about from one situation to another; and next, the observations made by it are confined to that part of the heavens where the refraction is scarcely sensible. Refraction is a thing which, (from the uncertainty attending the calculation of it,) baulks us perpetually, and which it is very desirable to get rid of as much as possible.

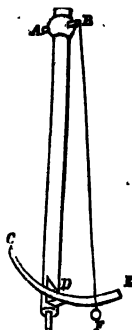


FIG. 19.

Now then, the way in which this instrument is used, in order to ascertain the form of the earth, is as follows: we take our Zenith Sector to Shanklin Down and to the Shetland Islands. Now, consider for a moment. What do I mean by the earth and water being curved? The direction of the vertical is perpendicular to the surface of water; and therefore, if the water be curved, it is connected essentially with the circumstance that the direction of the vertical is varied, or that the direction in which the plumb-line hangs is not the same at different places. Therefore, if the earth, Figure 18, be curved, as we suppose, and as previous rough considerations have given us reason to think, the plumb-line at A would

hang in the direction CGF, and that at B in the direction *c g f*. The place of the star, however, which I observe, is unaltered. The telescope is to be pointed in the same direction, whether we use it at Shanklin or Balta: or the line CD is parallel to *c d*. Suppose, therefore, I have gone through the observations in the way I have described, by observing what part of the limb of the Zenith Sector is crossed by the plumb-line; I get different parts of the limb in the observations at these two points. When I am observing the star at Balta, the plumb-line crosses at *g*; when I am observing it at Shanklin Down, the plumb-line crosses at G. Thus we obtain the difference of the direction of the vertical at the two places.

Now, then, I have arrived at something which I can use for taking the dimensions of the earth. In the way that I have described I have the inclination between the line, which is perpendicular to the surface at A, Figure 20, and the line which is perpendicular to the surface at B. If I continue these two lines downwards until they meet at a great distance below, as at H, I shall get a centre from which I may make a sweep to describe the curvature of this part; or, in other words, a centre, about which I may describe a circle passing through A and B, and such that its arc AB shall be exactly as much bent as the line AB on the earth's surface. If the earth be spherical, or round, it is plain that these lines come to the earth's centre, and the distance AH which I have found is the semi-diameter of the earth.

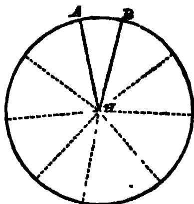


FIG. 20.

If you take numbers, you will see how we assume this to be effected. Suppose the measure of AB is 830 miles. Suppose I find that the directions of the two vertical lines AH and BH in the two places A and B make an angle of 12 degrees. You will remember what a degree means. It is not a measure of length; it is a measure of inclination of these two lines. I have to pass over a distance of 830 miles, in order to get from one place to another, where the direction of the vertical changes 12 degrees. From that I infer that the curvature of the earth is such, that I have to pass over 69 miles to find the distance of two places whose verticals are inclined one degree. Having got that, it is easy to find what is the semi-diameter of the circle which you must sweep, in order that that distance of 69 miles may give one degree of inclination of the two lines, drawn from the centre to the ends of the 69-mile arc. Making the calculation, you find the semi-diameter is about 4000 miles. And this is the way in which the measure of the earth was ascertained in the first instance. The first accurate measure was made in Holland, by a man named Snell; the next by a celebrated man, Picard, in France.

Shortly after this, Sir Isaac Newton's theory of gravitation was broached. He predicted, as a result of theory, that the earth would be ascertained not to be round, not spherical, but spheroidal, or flattened, turnip-shaped. It was a matter of importance to verify this. The first expedition for this purpose was made by the French Government, under the Kings of France; and all honour be to the French for the part they took in this matter! Many of you are aware that Guizot, the late Prime Minister of France, before he was appointed Minister of the

Crown, was Professor in one of the French Colleges. He gave lectures on the History of Civilization, and he maintained that France had been the great pioneer in science; that civilization generally had originated in France. I believe that in matters of science it is as stated by Guizot. When the question of the figure of the earth came to be debated, two celebrated expeditions were made under the auspices of the French Government: the first great scientific expeditions ever made in the history of the world. The party composing one expedition was sent to Lapland, to make a triangulation in the way I have described, and to make corresponding observations with the Zenith Sector, beginning at Tornea, at the head of the Gulf of Bothnia, and carrying the survey northward for about fifty miles. Another party was sent to Peru to make a similar measurement, but about two hundred miles in length. And no two expeditions ever rendered themselves more justly celebrated than these. Now, observe the results. In Figure 21, AB represents the Lapland measure, *a b* the Peruvian measure. It was found, that in Lapland they had to go 69¾ miles, or something like that, in order that the directions of the verticals should change one degree. It was found in Peru that they had to go only 69 miles, in order that the direction should change one degree. From this, it follows that the verticals AH and BH in Lapland meet at a point H, whose distance from A or B is about 4000 miles; and that the verticals *a h* and *b h* in Peru meet at a point

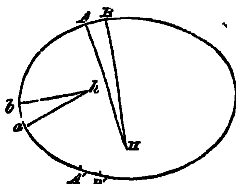


FIG. 21.

h, whose distance from *a* or *b* is about 3950 miles.

What is to be inferred from this? We have said that the estimation of the semi-diameter of the earth, supposing it to be a sphere, would depend on the distance you have to go, in order that the direction of the vertical might be altered by one degree. We have to go further in the northern measure than in the equatorial measure. It would seem at first sight, as a consequence, that the earth was not turnip-shaped, but egg-shaped; and this was maintained by many respectable people at the time. On consideration, it appeared that this was not a correct inference. And the reasons were these: when we assume that the earth is spheroidal, not spherical, then, inasmuch as we mean by the direction of the vertical "the direction of a line perpendicular to the surface of the water," the direction of the vertical will not go to the earth's centre at all. It is necessary to consider something different, and that is, that the measures which we have obtained, give us information of the curvature of different points of the earth. They tell us that at AB the curvature is little, but that at *a b* the curvature is very sharp. Altogether, when properly considered, they lead us to the inference that the form of the earth is something like the oval in Figure 21; that it is flatter at the Poles, and sharper in its curvature at the Equator. The rule which theory gave was, that the earth would be spheroidal; that is, that its form would be that which is produced by the curve called the ellipse,* Figure 21*a*, revolving round its shorter axis BB'. Adopting then the

* The nature of this curve is more fully explained in the next Lecture.

supposition that the earth is spheroidal, it was a

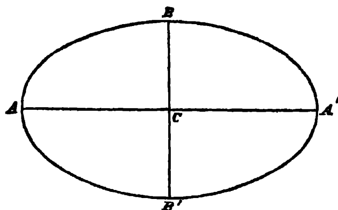


FIG. 21*a*.

matter of calculation to determine from the geometrical properties of the ellipse, what would be the proportion of the two axes, AA′, BB′, of the earth, which would make the proportions of the curvatures at AB and *a b* similar to those determined from the observations. It was inferred that they were in a proportion something like 299 to 300.

Since that time, extensive measures have been taken on other parts of the earth. At the Cape of Good Hope, a measure was made by Lacaille, a Frenchman, in the first instance. At the present time, I am happy to say, that this measure has been repeated and much extended, under the direction of the British Government, by Mr. Maclear, the Astronomer at the Observatory of the Cape of Good Hope. In England, the arc from the Isle of Wight to the Shetland Islands, to which I have several times alluded, has been measured. In India, an arc, extending from Cape Comorin to the neighbourhood of the Himalaya Mountains, has been measured under the direction of the East India Company. In Russia the measurement of an arc is going on at the present time, extending from the mouth of the Danube to the North Cape. It will form one of the best

means of determining accurately the dimensions of the earth. There is also one measure which is worth mentioning, on account of the extraordinary times in which it was effected. It was the great measure extending from Dunkirk, in France, to Barcelona, in Spain, and which was afterwards continued to Formentera, a small island, near Minorca. It is worth mentioning, because it was done in the hottest times of the French Revolution. We are accustomed to consider that time as one purely of anarchy and bloodshed; but the energetic Government of France, though labouring under the greatest difficulties, could find the opportunity of sending out an expedition for these scientific purposes; and thus did actually, during the hottest times of the revolution, complete a work to which nothing equal had been attempted by England.

Now, from all these measures put together, we are able to infer a proportion of the axes of the earth, and we are able to try whether all these different measures agree well with the supposition that the earth is a spheroid. There is a quantity of mathematical calculations concerned in it; the problem is this: suppose the earth to be a spheroid, with axes in any proportion that we choose to try; then to calculate mathematically the length of the measure corresponding to the observed inclination to the vertical in different parts of the earth, and to find how nearly these calculated measures agree with the measured arcs; to ascertain whether they agree so nearly, that there is no discordance beyond what can be fairly explained by the circumstances of the observations. They come to this: the proportion of the two axes of the earth is as 299 to 300: the shorter axis of 41,707,600 feet would pass through the Pole, and

the longer one of 41,847,400 feet would pass through the Equator: and the measures computed on this supposition for different parts of the earth do agree well with the measures actually made.

There is another method used for the same purpose, founded on the observations of arcs of longitude, which has not, however, been used so extensively as the other. Suppose we place a transit instrument at Greenwich, and observe the time when a star passes the meridian; suppose we place a transit instrument on the coast of Ireland, and observe the time when the same star passes the meridian there. It will be found that they do not pass at the same time. Why? Because the earth is curved. Let figure 22 be

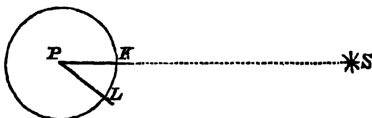


FIG. 22.

understood to represent the earth, with its Pole P turned towards the eye K and L, two stations, and S a star that is viewed; and consider what will be the positions of the transit instrument at K and L. If I place a transit instrument at K (which may represent Greenwich), the plane in which the instrument will move is the plane of PK, (see page 25,) and the instrument will catch the stars where they pass through that plane. If I erect a transit instrument at L, (which may represent a station on the west of Ireland,) its plane of movement will be PL, inclined to the plane PK. They both pass through the axis of the earth. The two planes will be inclined; and the stars will not appear to pass these

two planes, or through the two transit instruments, at the same time. The interval of time will depend entirely on the inclination of the meridian at these two places. If, then, we erect a transit instrument in one place, and another transit instrument at another place, and compare the times at which the same star passes the two transit instruments, we have the means of seeing how much the planes of the meridian are inclined. It makes no difference whether we suppose the earth to turn round so as to bring the plane of PL to pass through S, or suppose the star to turn round the earth, so as to make S pass through the plane of PL; the result is just the same. Now there are various ways* in which the comparison

* Since the delivery of these Lectures, the comparison of clock-times, by means of the Electric Telegraph, has been applied to the determination of the difference of longitudes, first in America, and subsequently in Europe. The mode of procedure is as follows: suppose it is desired to ascertain the difference of longitudes of Paris and Greenwich. At the moment when a star crosses the meridian of Paris the clock-time is observed; and simultaneously the observer, by means of the Electric Telegraph, transmits a signal to Greenwich; the time at which the signal arrives at the latter is noted by an observer there. If electricity travelled instantaneously from the one place to the other, the difference in hours, minutes, and seconds, between the times at which the signal was despatched and received, would be exactly the difference between Paris and Greenwich time; but, the velocity of the electric fluid being finite, the quantity in question is *less* than the difference between Paris and Greenwich time by the time occupied in the transmission of the signal: *less*, because Greenwich time is behind Paris time. Again, when the star reaches the meridian of Greenwich, a signal is in like manner sent to Paris by the observer: the difference in hours, minutes, and seconds, between the times of its being sent and received is now *greater* than the difference between Paris and Greenwich time by the time occupied in the transmission of the signal: *greater*, because Paris time is before Greenwich time. The mean between

of the times may be effected. One of them is by the use of an instantaneous signal of light. If I fire gunpowder on a high mountain, and if my assistants observe, from the two places where the instruments are placed, the time when the flash of gunpowder is seen, I can compare the clock at the two places. One person observes the clock-time at the one place when the flash occurs, and another observes the clock time at the other place when the flash occurs; and therefore, as soon as a letter can be sent by post from one place to another, I know how much one clock is faster or slower than the other. Another method is, by conveying watches, or small chronometers from one place to another. For instance, an expedition was arranged by myself some years since, to observe the difference of time between Greenwich and Valentia, on the south-west coast of Ireland. I had thirty chronometers carried backwards and forwards more than twenty times from Greenwich to Valentia, to compare the clocks. The chronometers were conveyed by railway carriages, by steam boats, by mail coaches, and by Irish cars, between Greenwich and the west of Ireland. By means of these I was enabled to compare the clocks at the two places; and by transit observations made with the assistance of these clocks, and with proper calculations, the times of the transits of the stars were compared, and therefore, I got the inclination of the planes of the meridian. By a survey-triangulation, extending in the east and west direction from Greenwich to Valentia, the distance was known in yards; and, knowing the distance, and knowing the inclination of the planes, the whole circumference of the parallel

the two quantities thus determined by observation, is the difference between Paris and Greenwich time.

of latitude passing through Greenwich was easily computed. Then we have to examine whether this circumference corresponds to the circumference calculated on the supposition that the earth is a spheroid, with a shorter axis of 41,707,600 feet, and a larger axis of 41,847,400 feet, and it is found to correspond well.

By means, then, of meridional measures by triangulation, and the Zenith Sector, and by means of east and west measures by triangulation, and observations with the transit instruments and comparisons of clocks, we have got sufficient information upon the form of the earth. Now, observe the very important conclusions to which that leads. In the observations given in the former lecture, we found that the whole of the heavens appeared to revolve, and we say, either the heavens revolve in the direction from the east, through south, to west; or the earth revolves in the direction from west, through south, to east. Which of these is the more likely? Astronomers agree without exception, that it is the earth which revolves. And I will tell you why. I dare say every person whom I see here has been brought up in the belief that the earth does turn round. But, I ask, if they had not been brought up in that belief, whether they would believe it now from what I am telling them? I do not think they would. Amongst all the subjects of natural philosophy presented to the human mind, there is none that staggers it so effectually as the assertion that the earth moves. We must not be uncharitable, then, towards people in the middle ages who did not believe it. To think that the solid earth moves, that the solid ground is going round at the rate of one thousand miles an hour, do you believe it? I will endeavour to give you grounds for the belief.

In the first place, I must say that even the

astronomers of antiquity had got a rough notion of the distances of some of the celestial bodies. But one will do for our present purpose. The moon is a long way off. There is a phenomenon of the moon observed frequently, in the interpretation of which there can be no mistake, namely, eclipses of the moon. We see that the moon, in her motions through the stars, dips into something which obscures her. There cannot be a doubt that it is the shadow of the earth. The moon goes into this shadow on one side, and comes out of it on the other side. The time which the moon occupies in passing through this shadow is, roughly speaking, four hours. The moon, then, is at such a distance that in passing through the shadow of an object as big as the earth, she is occupied only four hours. The moon, therefore, in her course describes the breadth of the earth in four hours; in one day she describes six times the breadth; and as thirty days is a rough measure for the time of her revolution, she describes in one revolution 180 times the breadth of the earth, and therefore the whole circumference of the moon's orbit is something about 180 times the breadth of the earth, and the diameter of the moon's orbit is about 60 times the breadth of the earth. Therefore the moon is distant from us by about 30 times the earth's breadth.

But there are other facts founded on observation. The sun is further off than the moon. There are phenomena called eclipses of the sun. We know that these correspond to times when the moon apparently approaches the sun; they are undoubtedly caused by the moon passing in front of the sun. Again, the stars are further off than the moon; because the moon passes in front of the stars,

producing what is called occultation. But they are not only further off than the moon, but they are a good deal further off than the moon; and the reason for knowing it is this: suppose one object is close behind another, then the more distant object will either be hidden by the nearer, in whatever part of the earth we may observe it, or will not be hidden at all. But, suppose the more distant object to be a long way off, then, when it was hidden from one part of the earth, it would be visible to another part of the earth. Now, that is the case with regard to the eclipses of the sun and occultations of the stars. If we examine into the appearances of an occultation, as seen at different parts of the earth, in some parts a star is hidden by the moon, in others it is not hidden at all. If we examine into the circumstances of a solar eclipse, as seen at different parts of the earth, in some parts the moon is seen on the north side of the sun; in other places the moon covers the sun; and in other places the moon is seen on the south side of the sun. It follows that the sun is a good deal further off than the moon. Thus it appears that the system of heavenly bodies which surrounds the earth is of considerable size. The moon is far off; the sun and stars are much further off than the moon. The moon, therefore, is not a small body; and the sun (which in spite of its great distance appears so large) must be a very large body. The stars are either connected in one system, or are so very far off that their relative movements are insensible.

Now, is it more likely that this large frame of things is turning one way, or that this small earth is turning the other way? Anybody must see at once, from the magnitude of things, that it is most probable

the earth is turning round. And, as regards the stars, the mere circumstance of their seeming to move all in a piece is a strong proof that they do not move sensibly, but that the earth moves. If you apply the same reasoning to any ordinary sublunary considerations, you will be struck at once with the conclusion. If I am sailing in a ship on the open sea and see vessels moving about me in all directions, and if any person in the ship asserts that our own ship is at rest and that all the others are moving, there is nothing particularly unreasonable in it. But if I am entering into the Ipswich river, and I see not only ships moving, but every church and warehouse, and the solid banks which connect them, moving past me, and if a friend at my elbow should say, "you are not moving, but all these solid things, churches and houses, and fixed objects and banks, are in motion," I should consider him to be a madman. The argument is precisely the same as applied to the heavens. If we had nothing but the sun and moon turning about in various ways, even then, and remarking their great size and their great distance, and the great speed with which they must be supposed to turn, (for the moon must be supposed to move at the rate of 60,000 miles an hour, and the sun very much quicker,) their daily revolution round the earth would be very unlikely. But when we have things of such an immovable character as the system of the stars, (like that of the banks of a river, or the solid erections which are there visible, as compared with our small sailing ships,) then the reason of man tells him at once that these things must be things of a fixed character—and that if these things be things of a fixed character, it is we who are turning and the earth which goes round. This is reasoning which ought to be received, and I cannot see why

it was not received by those who were able to reason on the matter in more distant times.

But when the telescope was invented, fresh objects presented themselves for contemplation, and new arguments were furnished. We then obtained a sight of the planets, particularly of Jupiter. We saw that he is a spherical, or rather spheroidal planet, like the earth, but probably much bigger. We can see spots upon Jupiter, and by these we can ascertain that he revolves in a much shorter time than the earth, or in about ten hours. Now, the knowledge of these things in later days has become a very strong argument indeed. Jupiter is a large planet that turns on his axis, and why do not we turn? We are very much alike in our general character.

But, finally, we come to another observation, founded on our determinations of the figure of the earth. We have found, from measures, that the earth is flattened at the Poles, or turnip-shaped. This leads, then, to the question—is that connected with the rotation of the earth? Most certainly it is. If we take anything circular which admits of a change of form; if, for instance, we mount a hoop, as in

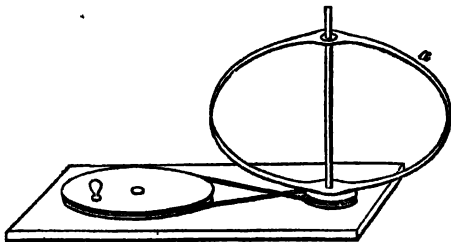


FIG. 23.

Fig. 23, in such a manner that we can make it revolve

rapidly, and whirl it round; then as soon as the motion of rotation takes place, the hoop becomes flattened.

From all these considerations then, put together in proper order, we infer, as a matter of positive certainty (however hard it may be at first for our minds to receive it), we infer as a matter of certainty that it is the earth which revolves.*

I shall now proceed with the next subject—the apparent motion of the sun amongst the stars. I make a point of entering upon this subject at the present time, that I may address to you some rough observations for your guidance in the enquiries which this subject involves. I shall explain to you the evidence which is within your own reach, and which proves that the sun apparently moves through the stars. I have not yet specially alluded to the sun in speaking of revolution; my remarks on that point referred only to the stars. I am now, however, going to speak of the sun a little, though it is necessary to have the stars to begin with, as fixed points in the heavens.

Everybody knows the leading difference between summer and winter; we know that the days are longer in the summer than in the winter. If you consider for a moment something else which you know and have remarked, you will see this, that the position of the sun is different in summer from what it is in winter. In summer, the sun at noon-day is high in the heavens; in the winter at noon-day he is

* Since these Lectures were delivered, M. Foucault has devised some experiments for rendering the rotation of the Earth visible to the eye. An account of these will be found in the Appendix.

low. In summer the sun is a long time above the horizon, and a short time below; in winter he is a short time above the horizon, and a long time below. In summer, the sun rises north of the east point, and sets north of the west point; in winter he rises south of the east point, and sets south of the west point. These observations are all explained by saying that in summer the sun is nearer to the North Pole, and in winter nearer to the South Pole. And this you can verify without the smallest trouble, from your own observations.

Now, I will offer a few words in regard to the stars visible at different times of the year. I will suppose that at eleven o'clock at night you look out to see what stars are on the meridian. On the first of January, if you look out at eleven o'clock at night, you will see the grand constellation Orion on the meridian; Aldebaran and Pleiades are west of the meridian. The bright star Sirius has not yet come to the meridian. I now suppose that you look out, in the same way, on the first of February, taking the same hour. On the first of February, at eleven o'clock at night, you see that Aldebaran and the Pleiades are setting in the west; that Orion has got a good way to the west; and that Sirius has passed the meridian a little way. There are three conspicuous stars near the meridian, well known by everybody who is accustomed to look at the constellations; namely, the pair of twins, Castor and Pollux, and Procyon, or the Little Dog. On the first of March, at eleven o'clock at night, you again look out; Castor, Pollux, Sirius, and the Dog Star, have then gone away to the west; Orion is now setting; and we then have the stars of the constellation Leo, which is a bright constellation, approaching the

meridian. On the first of April, at eleven o'clock at night, you again look out, and you see what is the state of things. The constellation Leo then has almost passed the meridian, except the bright star B (Beta) Leonis. On the first of May, at eleven o'clock at night, you again look out. You then observe that Leo and the whole set of stars are travelling away to the west; and then we have the bright star Spica in the constellation Virgo on the meridian; and we have the great red star Arcturus approaching the meridian. In the following month, on the first of June, at the same hour of the night, we again look out. We find that Spica has gone away to the west; that Arcturus has passed the meridian; and that other less conspicuous stars are on the meridian. In the same manner, if we go through the other months of the year; if, at eleven o'clock at night, on the first day of every month, we watch the appearances of the stars on the meridian, and compare them with those of the preceding month, we find that, from one month to another, they all travel on in the same direction towards the west.

Now, what is the inference? Is there any peculiarity in the motions of the stars? No, it is the motion of the sun. Our hour of eleven at night is referred by habit to the motion of the sun; and thus, when we speak of eleven o'clock at night, we mean that the sun is in a certain position; and, therefore, that the stars have moved in a direction from east to west, with respect to the sun.

But this may be interpreted another way. Regarding the stars as fixed objects, we get this: the sun travels round in the direction from west to east among the stars. Besides this, we have found that he travels in such a manner, that he goes nearer the

North Pole in summer, and further from it in winter. These are the general facts deduced from our observations. But, supposing we do not trust to them; supposing we make use of the Transit Instrument, and the Mural Circle. By the former we observe how long the sun is passing the meridian after a star; we find it is later and later every day; that every time the sun comes to the meridian, he has travelled (with respect to the stars) towards the left or towards the east, in such a manner, that, in the apparent diurnal motion, or passage, he is later and later, with respect to the stars, every day. Suppose that with the Mural Circle we observe the altitude of the sun every day when he passes the meridian; we find that he is nearer to the North Pole in summer than in winter. Thus, the sun travels to the left, and at the same time changes his distance from the North Pole.

Now, putting these things together, if we were to dot on the globe a succession of observed places of the sun, we should find they would follow each other in a curve, like that marked on Figure 24, (see Frontispiece,) which represents two views of a celestial globe, on opposite sides. This curve is called the *ecliptic.* It is convenient now to refer our description of this curve to the equator, which is the great circle on the globe equally distant from both Poles. Now, we find that the sun's path, or ecliptic, crosses the equator at two points. One of these is called the First Point of Aries, and this is the sun's apparent place at the beginning of spring; from this point the sun appears to approach nearer and nearer to the North Pole until midsummer. He then appears to recede from the North Pole, and crosses the equator in the first point of Libra, at the beginning of autumn, and approaches the South Pole till

mid-winter; after which he turns towards the first point of Aries.

Now, upon examining that curve or ecliptic, we find that it is one of those circles which are called "great circles"; they divide the globe into equal halves. I have stated this as a thing which would be one of rough evidence; that if, by means of the observations I have mentioned, (namely, how long the sun is in passing the meridian after a star, and what is its angle of elevation when it passes the meridian,) if by means of these we mark on a globe the successive places of the sun, we shall find all the successive points in a curve, such as I have described. When I say that it goes on the globe in such a curve, you must understand that there are ways of computing these things; that having got the interval of time between the sun and a star in passing the meridian, and the angular elevation of the sun when it does pass the meridian, it is possible, by computation, to find whether it is going on in such a curve as I have described; but that these computations, though they amount to the same thing as dotting the sun's places down on the globe, are a great deal more accurate. It is thus found that the ecliptic, or apparent path of the sun through the stars, is accurately a great circle, or one which divides the globe into equal halves.

The first point of Aries, as I have said, is one of the places where the ecliptic crosses the equator; it is the point which the sun passes at the beginning of spring; it is not marked by any star, or fixed object of any kind. Though it is an imaginary point, yet, from having a series of places of the sun, defined by the difference of times at which the sun and a bright star pass the meridian, we can tell as exactly where

that intersection is, as if it were marked by a conspicuous point in the heavens.

Now, the statement that the sun appears to move in a great circle on the globe amounts to this: that the sun appears to move in a plane which passes through the earth. For all globes represent the stars as they would appear if the observer were at the centre of the globe; and a great circle is one whose plane passes through the centre of the globe. If we found that the sun was describing one of the smaller circles on the globe, then we should say the sun was not moving in a plane round the earth. But, as it appears to describe a great circle, then we can assert that the sun appears to be moving in a plane round the earth. Now we come to the question. Cannot we explain this differently in another way? Is it certain that the sun is moving in a plane round the earth, or is it certain that the earth is moving in a plane round the sun? Either supposition will do. At the present moment we have no evidence to guide us; we have nothing to tell us whether the sun is moving round the earth in a plane, or whether the earth is moving round the sun in a plane. But we shall shortly have evidence on the point.

In the meantime I will mention this: that there is no inconsistency in supposing that the earth does move round the sun. As regards the position of the earth's axis there are two suppositions: either that the earth remains with the place of its centre unmoved and with its axis in a certain position, and that the sun goes round at a certain inclination to that axis, thereby causing the change of seasons; or else, if the sun is fixed and the earth goes round the sun, the position of the earth's centre is changed with regard to the sun, but the position of the axis must

remain the same relatively with itself. This is a point of great importance. You will remark that all these conclusions are derived from observations of the stars; that observations of the Polar Star are used to determine the Pole round which the heavens appear to revolve; or on the more rational supposition, to determine the point of the heavens towards which the axis of the rotation of the earth is supposed to be directed; summer or winter, the Polar Star is the Polar Star still, and guides our observations; summer or winter the axis of the earth is directed towards that same part of the heavens where the Polar Star is seen. Therefore, if the earth does revolve round the sun, we must suppose it to revolve in such a manner, not that its North Pole is always inclined towards the sun, but that it is sometimes inclined away from the sun; in point of fact, the motion will be imitated by the motion of the earth in the simple orrery,* represented in Figure 25.

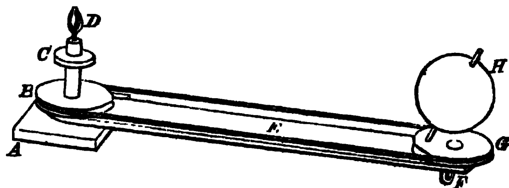


FIG. 25.

* In Figure 25, A represents a block to be screwed to the table; B, a grooved pulley fixed firmly to A, and having no motion whatever; C, a stand supporting a lamp D, which represents the sun; E, an arm which turns round the axis of B, running upon a small roller F; G, a grooved pulley of the same size as B, which turns in E and carries the axis of the earth H at the inclination 23½ degrees to the vertical. A band is passed round B and G. Then, when the earth is turned round the sun, its axis moves always parallel to itself.

Everybody who knows the character of the seasons at the different parts of the earth, will know that this representation is entirely in conformity with their respective changes. Therefore, we must make the following supposition: that if the earth does turn round the sun, its axis of rotation remains parallel to itself, having no reference whatever to the sun, whilst the earth is going round the sun; or, to express it in other words—that the earth has the power of preserving its axis of rotation in the same position all the time.

Now, it is remarkable, as a mechanical fact, that nothing is so permanent in nature as the axis of rotation of anything which is rapidly whirled. We have examples of this in every-day practice. The first is the motion of a boy's hoop. You will think perhaps that this illustration is of rather a superficial character, but it is of great importance. What keeps the hoop from falling? It is its rotation. I cannot now enter upon the explanation of this, which is one of the most complicated subjects in mechanics. Another thing pertinent to the question before us, is the motion of a quoit. Everybody who ever threw a quoit knows, that to make it preserve its position as it goes through the air, it is necessary to give it a whirling motion. It will be seen, that while whirling it preserves its plane, whatever the position of the plane may be, and however it may be inclined to the direction in which the quoit travels. Now, this has greater analogy with the motion of the earth than anything else. Another admirable illustration is the motion of a spinning top. I hope I shall not be thought derogating from the dignity of science by giving such an illustration. The greatest mathematician of the last century, the celebrated Euler, has

written a whole book on the motion of a top, and his Latin Treatise "*De motu Turbinis*" is one of the most remarkable books on mechanics I ever read in my life. The motion of a top, I repeat, is a matter of the greatest importance; it is applicable to the elucidation of some of the greatest phenomena of nature. In all these instances, there is this wonderful tendency in rotation to preserve the axis of rotation unaltered.

Now, from all these circumstances, we see sufficient reason to explain how, whilst the earth is going round the sun, its axis of rotation should remain parallel to itself without being disturbed, that is to say, that the position of the axis has no respect whatever to the sun. Whatever the position of the axis of rotation be, the earth will travel through space keeping that axis of rotation in the same position with regard to the distant stars. Having reached this very important point in the science, I will stop for the present.

LECTURE III.

Recapitulation of Lecture II.—Apparent motion of the planets. —Greek Theory of Planetary Motions.—Epicycles, Deferents, &c.—Copernican Theory.—Kepler's Elliptic Theory. —Theory of Central Forces.—Laws of Motion.—Composition and Resolution of Forces.—Motion of a Planet in its orbit deduced from these laws.—Measure of distance by Parallax.

The subject of the lecture of yesterday evening was the dimensions, the figure, and the rotation of the earth. I then thought it necessary to put before you some details of evidence relative to the rotation of the earth; and in again entering upon that subject, but not exactly in the same order in which I took it last night, I shall feel it necessary to remark that it is highly important, in beginning a subject like this, to divest yourselves, as far as you possibly can, of notions acquired in the ordinary routine of education. Every person here has, without doubt, been brought up in the belief that the earth is in motion; and, because they have had this belief instilled into their minds from their earliest infancy, they may have concluded that it is necessarily and obviously true. This is a thing most dangerous, and instances are not wanting to prove that in every branch of science, absurdities have arisen from it. I may mention one which just at this moment occurs to my mind, and which influenced the mind of one of the earliest philosophers of the Royal Society of

London. At the beginning of that Society, as at the beginning of most Societies, although some care might have been taken that no absurdities should creep in, it was difficult to avoid them entirely. And in a paper which was intended to prove the truth of the Copernican theory, for which purpose the writer (I do not remember why) thought it necessary to prove that the stars are at tolerably equal distances, he begins by establishing the latter proposition by means of the following assumption: "Now we all know that hell is the centre of the earth." It seems perfectly absurd at the present time that anybody should start with a proposition like that to work out a physical theory. Yet it is equally absurd to assume at once that the earth is in motion, and for that reason I have been anxious to convey to you the evidence by which it is proved generally that the earth is in motion. And I shall now proceed to recapitulate, in as few words as I can, the main points of what was said yesterday in regard to the earth.

I endeavoured to point out to you the method of measuring the earth, and I told you that we wanted the means of measuring hundreds or thousands of miles. In some instances it is obvious that to measure a long meridional arc, in the most direct line that the earth's curvature permits, is an impossibility. The way is, to measure a short line which I call the base line, being a few miles in length; and great trouble is necessary to give even to this measurement the requisite accuracy. When the base is measured, we plant theodolites at its two ends, and by means of these we observe a signal upon a hill, or any other distant place. It is then usual to carry a third theodolite to the signal station: this is not absolutely necessary, but it is done as a matter of prudence, to

verify the observations in case of suspicion of error. Now, having the base of the triangle, and the two angles next that base, there is then no difficulty in laying them down on paper, or in calculating the other sides of the triangle. Then we may use one of these computed sides as a measured base, and if from its two ends we can see some other signal, we can observe it with our theodolites, and compute its distance in the same manner: and in this way the triangulation goes on. Thus, in Figure 17, a series of triangles was formed, extending from Shanklin Down, in the Isle of Wight, to Clifton, in the south of Yorkshire.

I called your attention particularly to the remark, that this is the first instance in which we use the yard measure, which is done by actual application in measuring the length of the base, and by computation from this in measuring the length of every one of the sides of the triangles; and thus we do really get the different distances in the triangulation, by the use of a yard measure.

I then mentioned to you that, supposing we had extended the survey over a very long distance, the next thing was to make use of the Zenith Sector, Figure 19, which consists in its important feature of a telescope with a graduated arc CE attached to it, turning on two pivots AB, and with a plumb-line suspended from, or passing over, one pivot B, and crossing the graduated arc. (The Mural Circle may be used for the same purpose, but the Zenith Sector is rather more convenient.) We have then to consider that, whatever the form of the earth may be, using the expression as applying to the fluid part of the earth, we must suppose also, from the nature of a fluid, that the direction of the plumb-line is,

perpendicular to the surface, and therefore, if we suppose the earth to be fluid, the plumb-line will be always perpendicular to its surface. If, then, we plant the Zenith Sector at A, Figure 18 or 20, the plumb-line will hang in a direction perpendicular to the surface at A. But if at B, the plumb-line must hang in the direction perpendicular to the surface at B: therefore if at A we observe a star nearly overhead, then the plumb-line will fall over the point G of the arc; but if we carry the Zenith Sector to B, and turn the telescope to the same star, the plumb-line will fall on the point *g* of the arc. Inasmuch, therefore, as the telescope, from being directed to the same star, which is excessively distant, takes the same direction in different places; and, inasmuch as the plumb-line takes different directions in different places; by means of these we get the variable positions of the plumb-line referred to the invariable position of the telescope. I then called your attention to Figures 20 and 21, and said, if we suppose the vertical lines at A and B to be carried down till they meet at H, the angle made by these two verticals, or by the two plumb-lines, would be the difference of the Zenith-distances of the star as observed at A and B, that is to say, the difference of the two angles made by the telescope with the plumb-line, first at A, and secondly at B. Having got the angle of these two lines, AH and BH, and the length of the line AB which connects their ends, we are enabled to calculate the length AH or BH, or the number of miles of distance of their intersection H. This is, in point of fact, the semi-diameter which must be taken in order to sweep the curvature of the arc AB; or, if you please, we may put the result in this shape: we may say that, having travelled 830 miles, we find the inclination of

the verticals to be 12 degrees, and therefore we should have to travel 69 miles to make the inclination of the plumb-lines one degree, and that is commonly expressed by saying a degree on the earth's surface is equal to 69 miles.

I then pointed out to you the principal lines which have been accurately measured. All these lead to the conclusion, that towards the Poles of the earth you have to travel 69½ miles in order to pass over the space where the direction of the vertical changes by one degree, but that near the equator you have to go only 68¾ miles, in order to pass over the space where the direction of the vertical changes one degree.

Now, I call your attention to the interpretation of this circumstance: it shows that the earth's dimension is greater in the direction of the equatoreal axis, as shown in Figure 21. It is necessary to consider that the direction of the vertical is not to the centre of the earth—it is perpendicular to the surface; and the intersection of the two verticals at H or *h* does not give the distance from the centre, and does not depend on the distance from the centre, but on the curvature at each place. And inasmuch as, when near the Pole, you have to travel the greater distance, in order to go through the same change of the direction of the earth's surface, it proves this: that the earth is less curved at the Pole than near the equator, and that you come to a shape something like Figure 21. About AB the surface is comparatively flat; about *ab* the curvature is sharpened; and at the Cape of Good Hope, or about A′B′, it is flattened again. So that we come to the conclusion, so far as our measures go, that the form of the earth is somewhat turnip-shaped, or is what we call an oblate spheroid.

But there is another kind of evidence derived from the measure of arcs of longitude, obtained by ascertaining the difference of time at which the transits of stars are seen at different places. Thus, in Figure 22, the star S is not on the meridian at the two places K and L at the same moment of time. Now, we want to measure the difference of time at which a star passes over the meridian at the two places: and this is done by using some means of comparing the times of the two clocks at the two stations—thus ascertaining how much one clock is before or behind the other. And inasmuch as we have the transit instrument, we can determine the absolute times at which the same star passes at both stations; so that by observing transits of the star with the clock at one place K, and by observing transits of the same star with the clock at the other place L, and comparing the clocks, we have the means of ascertaining the absolute difference of time of transit; and when we have done that, we can tell how great a fraction of the revolution of the earth has been performed. Clocks may be compared by observation of instantaneous signals, such as the flashes of gunpowder fired on elevated stations. There is one long arc, commencing in the neighbourhood of Padua, in Italy, crossing the Alps, and terminating at Marennes, near Bordeaux, in France. Intermediate places, A,B,C,D, &c., were chosen for clock stations, in such positions that one set of signals on an intermediate mountain could be seen both at A and at B; another set of signals on another mountain could be seen both at B and at C, and so on to the end of the arc; and thus clock A was compared with clock B, clock B with clock C, &c., to the end of the arc. Another method of

performing the same operation is that which has been used on the arc from Greenwich to Valentia, in the south-west of Ireland. Thirty chronometers were carried backwards and forwards twenty-two times, and in this manner the clocks were compared with great accuracy. Surveys have also been carried on by triangulation, connecting the extreme east and west stations (as Padua and Marennes, or Greenwich and Valentia) on the same principles as the surveys in the north and south direction. Thus, then, from the comparison of the clocks, and the observations of transits, we have the means of knowing the fraction of a revolution which the earth has performed, from the time when a star passes over the meridian of one place, to the time when the same star passes over the meridian at another place. Thus the difference of these times at Greenwich and Valentia was found to exceed 41 minutes 23 seconds. Now, the problem becomes this: if in 41 minutes 23 seconds so many miles pass under the meridian of the star, how many miles will pass under the meridian in 24 hours? This is a mere question in the rule of three. The whole girth of that particular part of the earth may thus be obtained.

We have thus got the measures of the meridian in various parts, giving us the length which it is necessary to go for a degree; we have got two grand measures of parallel (as they are called), and also some smaller ones, giving us the girth of the earth in different parts. The question then is, what sort of figure do they belong to? Do they belong to a spheroid? Upon trying this we find they do belong to a spheroid, so that by giving certain dimensions to the spheroid, the measures of all the different arcs will be very well represented. The diameter passing

through the Poles must be about 7899 miles; that passing through the equator about 7926 miles. Therefore we may consider it as established, that the form of the earth is spheroidal.

The next thing we have to consider is, what inference we are to draw from that, in reference to the movement of the earth. By a rough experiment it was shown, that if we take any circular substance that is susceptible of a change of shape, and whirl it round an axis, it will change from a circle into an oval; we think, therefore, that even supposing we had nothing else to guide us, there is good reason to infer, from the oval shape of the earth, that it does turn upon its axis. But, in addition to this, we see the sun, moon, and stars, every day turning from east to west. We know (by the duration of the lunar eclipses) that the distance of the moon is considerable; and (by the fact that solar eclipses and occultations of stars do not extend over all the earth) that the distances of the sun and stars are very much greater; we also see that the system of stars appears to move all in a piece; we judge it unlikely that these distant bodies should thus revolve round the earth every day. And we conclude that the apparent movement is caused by the earth's turning from west to east. It is worth mentioning, that the planet Jupiter, the largest planet of the system, turns visibly round its axis in a shorter time than the earth. You may suppose, then, that Jupiter is much more flattened by the velocity of his rapid rotation than the earth; and indeed you can see it at once with a telescope without the aid of a micrometer, the equatoreal being to the Polar diameter as 16 to 15, nearly—a proportion which makes the former measure 5000 miles greater than the latter.

I then spoke of the apparent movement of the sun amongst the stars; and in speaking of their movements, I endeavoured to impress upon you how much you can observe for yourselves. You can learn more by your own observations than by the lectures I can deliver, or by all the books you can read. In speaking of the apparent motion of the sun amongst the stars, I told you that in summer the sun is longer above the horizon and goes higher than in winter; the sun also rises more to the north in summer than in winter. Thus it describes a daily circle nearer to the North Pole of the heavens in summer than in winter; or, in other words, its place among the stars is nearer to the North Pole in summer than in winter. But there is also another set of facts, though not quite so familiarly known as these, which you can observe for yourselves. If you watch the appearance of the stars at a certain hour every night, you will find that these stars are to be found in a position on the night of one month different from the position they are in on the corresponding night of the following month. You will observe from one month to another (if you always look at the same hour of the night) that they will travel away to the west. These motions are referred to the sun, by our habit of using solar time; that is to say, at the same solar hour, or when the sun is at the same distance from the meridian, the stars are travelling away to the right; or, in other words, the sun travels away to the left amongst the stars. And at the same time, as I have mentioned, it changes its distance from the North Pole.

Now, I will endeavour to point out to you how this is more accurately observed. I remarked that the Mural Circle is used to determine how far the

Polar Star appears above the north horizon, at its highest and lowest positions, and thus to determine the height of the Pole above the north horizon. It is also used to determine how far the sun is above the south horizon; and from these two measures the sun's angular distance from the Pole is obtained. When we were speaking of the transit instrument, I described its use in this manner: suppose we observe the time when some well-known star passes the meridian, (for instance, the bright star of Aquila, adopted for that purpose by one of my predecessors, Dr. Maskelyne,) and also the time when the sun passes the meridian. The sun passes the meridian so many hours, minutes, and seconds after the star. We bring the place of the star on the celestial globe under the meridian, and then we turn the globe through the corresponding angle; then we know that the sun's place will be somewhere under the meridian. At the time that it passes, it has a certain elevation above the horizon, and therefore a certain distance from the Pole, expressed in degrees and minutes, which is found by the Mural Circle: we take this number of degrees and minutes along the meridian from the Pole; and thus we find the place where the sun was at the time of observation. We make a mark on the globe at that place. We repeat these observations every day of the year; we get measures of the same kind; and we find a series of places such as those shown in Figure 24. When we come to examine all these as laid down together, which may be done roughly on a globe, or more accurately by calculation, we shall find that they are all lying in a great circle. You must understand what is meant by a great circle; it is a circle dividing the sphere into equal parts. Its plane therefore passes through

the centre of the sphere. Now, our use of the celestial globe or sphere is founded on the assumption that it is a representation of the heavens, on the supposition that the eye of the observer is at the centre of the sphere. This circle, then, in which the sun appears to move, being one whose plane passes through the centre of the sphere, or through the eye of the observer; it comes to this, that the sun appears to move around the earth in a plane, or that the earth moves around the sun in a plane.

If the sun moves round the earth, we have only to suppose that the earth stands with its centre stationary, but that it is whirling round its axis, and that the sun travels round and round. If you suppose that the earth travels round the sun, it is necessary to suppose that the earth's axis retains its parallelism to itself without any respect to the sun. Now, is it likely that the axis of the earth would remain parallel to itself without respect to the sun? In my former lecture, I called your attention to the motion of a quoit and a top, in order to show you the strong tendency which rotatory motion has to maintain the position of its axis unaltered. It is the quoit whose motion has the most striking analogy to the motion of the earth. The quoit is not impeded by contact with the floor as the top is. This has been made the subject of mathematical investigation, as well as of experiment, and the result of both is, that the earth, if revolving round the sun, would carry its axis of rotation always parallel to one line, as we see it.

To this I may add one remark. Geologists have observed that important changes have taken place in the climates of different parts of the earth. Some have supposed that the axis of the earth must have changed its position; but there is no greater impossibility

than that the axis of the earth should change its position. So strong is the tendency of rotation to preserve the position of the axis unaltered, both in parallelism to itself, and with respect to its position in the rotating body, that we may assert boldly that the earth's axis has always been in the same general position within the earth as when the earth first received motion. And its position, as regards the place among the stars to which it points, is affected only by the very slow motion called Precession of the Equinoxes (of which more will be said hereafter); and even this does not affect its inclination to the plane of the ecliptic.

Having got through this part of my subject, I will now proceed to speak of the apparent motion of the planets. The movements of the planets are extremely complex. At the present time,* Venus is what is called the morning star; she is to be seen before sunrise. If you watch her movements through the stars, you will see that her motion is in the same direction as the motion of the sun. You will remember what I said in regard to the motion of the sun: that it moves in regard to the stars, in a direction opposite to the movement of the hands of a watch. You cannot see the stars surrounding the sun though astronomers can see them with their telescopes; but by watching the stars from month to month, you find that the stars appear to move away from the sun towards the right, or that the sun moves among the stars towards the left. That is called direct motion. If you look at Venus at the present time, you will see that she is moving in a direct motion faster than the sun. She will go behind the

* 15th March, 1848.

sun, and after that she will be seen as an evening star; she will then be going away from the sun; she will go away for a certain distance, but more and more slowly, and the sun will be approaching towards her; she will become stationary; then she will turn backwards and seem to meet the sun.

Can we make reasonable theory to account for this? We can do it more easily if we refer the apparent motion of Venus to the sun and not to the stars. Venus sometimes passes the sun in going from left to right (relatively to the sun), and sometimes going from right to left (relatively to the sun); and her extreme angular distance from the sun towards the right is almost exactly the same as her extreme angular distance towards the left. The Greek astronomers began with a good assumption; they laid down at once the notion which they conceived must be the most natural and most proper, which was this: that every planet revolved in a circle. They then supposed that the earth is fixed, and that the sun moves. They supposed that a bar, or something equivalent, is connected at one end with the earth, and that on some part it carries the sun; and as they saw that the planet Venus was apparently sometimes on one side of the sun and sometimes on the other side, they said that the planet Venus moves in a circle, whose centre is on the same bar. Whether they have expressed themselves distinctly concerning this bar I cannot say, but all their notions of the position of the centre orbit of Venus come to the same thing. Then, suppose that Venus is revolving round the centre at the same time that the bar is moving, we then get a perfect representation of the apparent motion of Venus and the sun, as seen from the earth. These suppositions will be represented on Figure 26, by

supposing E to be the fixed earth; E *v* S *m* *n*, a bar turning in a circle, having one end fixed as at E; S,

FIG. 26.

the sun carried by it; *v*, the centre of the orbit in which Venus revolves; V being the planet Venus, connected with *v* by a bar (real or imaginary), and thus describing a circle round *v*, while *v* itself is carried on the bar round the earth.

They supposed that Mercury (see Me in Figure 26) revolves in another circle, and that its centre is on the same bar, but perhaps beyond the sun, as at *m*. They did not, however, pretend to judge exactly where these centres are; all that they were certain of was this: that the centre of the motion of each planet is on the same bar that supports the sun. Now, you may easily see that, on these suppositions, the planets being viewed from the earth, Venus is at one time to the right, and at another time to the left of the sun, and the sun is carried round the earth in one year. The same is the case with regard to Mercury.

With regard to Mars, they found out that its motion can be represented extremely well, by supposing that this same bar carries another centre as *n*, around

which Mars revolves as at *Ma*, carried by an arm so long that it projects beyond the earth, so that its orbit completely surrounds the earth, as well as the sun, in describing its whole motion.

It is, however, rather difficult for us to conceive how the centre of this motion can be carried. There is no bar that we can see. It was, however, necessary for them to suppose that there is a bar which is attached at one end to the earth, and which carries the sun, and carries also the centres of the motion of the other planets. It does appear strange that any reasonable man could entertain such a theory as this. It is, however, certain that they did entertain such a notion; and there is one thing which seems to me to give something of a clue to it: in speaking to-day and yesterday of the faults of education, I said that we take things for granted without evidence; mankind in general adopt things instilled into them in early youth as truths, without sufficient examination; and I now add, that philosophers are much influenced by the common belief of the common people. There is one passage in Herodotus, where he is endeavouring to account for certain phenomena in Egypt, which I have often read, and which, so far as I see, can only mean this: "That certain periodical winds do carry the sun from north to south, and that thus the change of seasons is produced." I think it likely that Herodotus (who was a learned man for that time) believed that the sun was something in the atmosphere little better than a cloud, perhaps not so important as an aurora borealis, and that it might be carried along by the winds. We know, also, that at a time not very distant from that, a Greek philosopher, named Anaxagoras, dissented from this notion, saying—"That the sun was solid, and as big

as the country of Greece," and that he was persecuted for saying so. Having these things before us, I am not much surprised that the Greek astronomers considered the sun as completely subordinate to the earth, and therefore supposed it to revolve round the earth; and when they had once adopted this idea, they were compelled to take the complicated and unnatural explanation which I have given of the motion of the planets.

The motions of the planet Mars, however, still presented some discordance, and there were some smaller discordances with regard to all the other planets. Then were invented those things, known by the name of epicycles, deferents, &c. of which the nature may be thus explained. By the contrivance of which I have previously spoken, (and which is represented in Figure 26), they found that the movement of the point M*a* at the end of the rod *n* M*a* would nearly, but not exactly, represent the motion of Mars. To make it represent the motion more exactly, they supposed that another small rod MN was carried by the longer rod, jointed at M, and turning round in a different time. To make it still more exact, they supposed another shorter rod carried at N, and that its extremity carried the planet Mars; and so for the other planets. Of all the complications of systems that ever man devised, there never was one like this Ptolemaic system. The celebrated King of Castile, Alphonse, the greatest patron of Astronomy in his age, alluding to this theory of epicycles, said "If he had been consulted at the Creation, he could have done the thing better." It was merely expressing his absolute inability to receive, as a possible explanation of nature, such a complexity of things.

But there was one consideration so simple, that it

seems astonishing that it did not occur to people before. When we suppose the earth fixed as at E, Figure 27, and take Venus (for instance) revolving

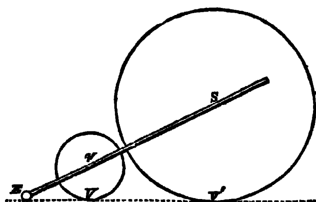


FIG. 27.

round a centre, we may alter the place of that centre and its distance from the earth as much as we please, and we shall then get the same appearances, provided we alter the dimensions of the orbit of Venus in the same proportion. As, for instance, in Figure 27, suppose E to be the earth, and suppose the small circle in which V is to be the orbit of Venus, the sun being at S; then, in revolving in her orbit, Venus appears to go to a certain extent to the right and to the left of the sun. But we might take any other point on the bar, even the point S itself, for the centre of the orbit of Venus, provided we give Venus a larger circle to revolve in. In the large orbit in which V′ is seen, Venus will appear (as viewed from the earth) to move to the right or left of the sun; and if we do but make the orbit large enough, it will, as viewed from E, appear to move just as much to the right or left of the sun as if it moved in the small orbit. We may then fix the centre of the orbit of Venus where we please. When we have got thus far, we may easily make another step. Suppose we

assume the centre of the orbit of Venus to be the same point as the centre of the sun: we shall not have so much complexity. Suppose now we assume also that the centres of the orbits of the other planets are in the centre of the sun: we have seen that we can thus account for the motion of Venus, by giving proper dimensions to her orbit; and we can do the same thing for Mercury, and for Mars, and for every one of the planets. Just observe the state of things we have got to, as in Figure 28; instead of having

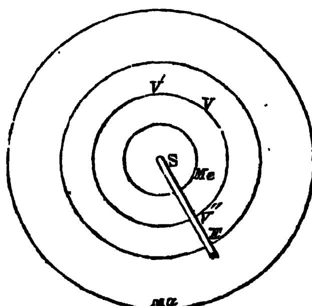


FIG. 28.

different centres of motion for different planets, we have got them all in the centre of the sun; and the sun turns round the earth, carrying the orbits of the planets with it. That is a considerable simplification. In this state, I believe, the theory was received by the great Danish Astronomer, Tycho Brahé.

But now, instead of supposing the sun to be travelling, being itself the centre of the other orbits, and by some imaginary power causing the planets to revolve round itself as their travelling centre, suppose

we say that the earth revolves round the sun, and that the sun is a fixed, or nearly fixed body, and that all the planets, including the earth, go round the sun; that is, in Figure 28, instead of supposing S with the whole system of orbits to travel round E, suppose M*e*, V, E, M*a*, and others, to travel in separate orbits round S. The appearances of the planets, as viewed from the earth, will be represented exactly as well as before. How it could then happen that a theory like that of the Greek astronomers could still be received as true, after the publication of the simple explanation which I have now given, is beyond my conception. It did, however, very much change the relative importance of the sun and the earth; it made the sun the most important body of all, and the earth one of the least important; and perhaps it was this which really occasioned the difficulty of receiving it. This great step in the explanation of the planetary motions was made by Copernicus, an ecclesiastic of the Romish Church, a Canon of Thorn, a city of Prussia. The work in which he published it is dedicated to the Pope. At that time it would appear that there was no disinclination in the Romish Church to receive new astronomical theories. But in no long time after, when Galileo, a philosopher of Florence, taught the same theory, he was brought to trial by the Romish Church, then in full power, and he was compelled to renounce the theory. How these two different courses of the Romish Church are to be reconciled I do not know, but the fact is so.

Soon after the time of Copernicus the telescope was invented by Galileo. One of the most important discoveries made with it was, that the planets do not always appear to be round, and that they obviously are not fully illuminated at all times. The

planet Venus puts on all the phases of the moon. When the planet Venus is at that part of her orbit at which, in conformity with our theory, she is beyond the sun, as at V′, Figure 28, we then see her as round as the full moon; and when the planet Venus is at those parts at which, in conformity with our theory, she is almost between us and the sun, as at V″, it is found, by observation with the telescope, that she then puts on the phases of a young moon. These are precisely the appearances that would be seen if the theory is true. This is a most important confirmation, which was wanting in the time of Copernicus, and which with us is so convincing, that any one who has seen Venus will not doubt the truth of the theory.

The great step made by Copernicus was the assumption that the sun is the centre of the motion of all the planets (including the earth). But he could not get rid of the epicycles. As in Figure 26, where Mars is carried at N, at the extremity of a small arm, jointed on to a longer arm and revolving round the joint, so it was still necessary to suppose that each of the planets, as well as the earth, was carried by a similar apparatus; and even this did not represent the movements with perfect accuracy. This was reserved for Kepler to explain, who—not so much from his own observations as by examining accurately the observations which Tycho Brahé had made of the planets, and especially the planet Mars, and comparing them with his own—ascertained that the whole would be represented to the utmost accuracy by supposing that Mars moves in an ellipse. It is impossible now to explain in a few words how Kepler came to that conclusion; generally speaking, it was by the method of trial and error. The number of

suppositions he made to account for the motions of the planets is beyond belief: that the planets turned round centres at a little distance from the sun; that their epicycles, deferents, &c., turned on points at a little distance from the ends of the bars to which they were jointed, &c. It is by this kind of investigation, by trial and error, that truth is established. The way in which he has published his adoption of this theory is very striking. After trying every device with epicycles, eccentrics, and deferents, that he could think of, and computing the apparent places of Mars from these different assumptions, and comparing them with the places really observed by Tycho, he found that he could not bring them nearer to Tycho's observations than by eight minutes of a degree. He then said boldly that it was impossible that so good an observer as Tycho could be wrong by eight minutes, and added, "out of these eight minutes we will construct a new theory that will explain the motions of all the planets." He then proceeded to explain the theory of motion in ellipses.

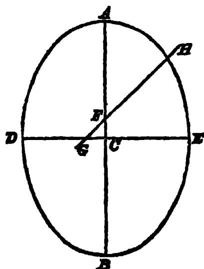


FIG. 29.

I shall now speak of elliptic motion. I must first state what an ellipse is. There are different ways of describing an ellipse. I dare say there are many mechanical persons near me who are acquainted with a carpenter's trammel. An ellipse, or an oval may be described in this way, in Figure 29. Suppose AB, DE, to represent the longer and shorter diameters of the ellipse, at right angles to each other; then, if we have a bar GFH, with two pins FG fixed

in it, so arranged that the pin F shall always move along the long diameter AB, and the pin G shall always move along the short diameter DE; then any point H of the bar describes an accurate ellipse. This is the principle used in carpenters' trammels and oval chucks. It describes an accurate ellipse, exactly similar to that described by another method, of which I am going to speak, but it has no relation to the various parts of the ellipse upon which I am going to remark. In Figure 30, if we stick a pin in a board at S, and call that point a focus; and if we stick another pin in the board at H, and call that a focus; if we then fasten a string by its two ends to these two pins, keeping it always stretched by the point of a pencil, as at P, and carry the pencil round, it will describe an ellipse. S and H are the two focusses of the ellipse; but in all the treatment of astronomical theory, we have only to do with one of them.

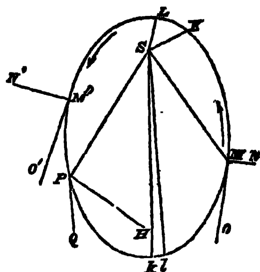


FIG. 30.

If the ellipse in Figure 30 be the orbit of a planet, S will be the place of the sun. The sun is at one focus of the ellipse described by every planet. Every planet describes a different ellipse. The degree of flatness of every ellipse is different for every planet; the direction of the long diameter of the ellipse is different for every planet; there is every possible variety among them.

Now, one of the important things that Kepler made out was this: that the orbits of planets are

ellipses. Another important thing made out was this: that the planets describing these ellipses move with very different velocities at different times. Each planet, when in that part of its orbit which is nearest to the sun, travels quickly, and when in that part which is furthest from the sun, travels slowly. The way in which he expressed the law of motion is this: if in one part of the orbit I draw two lines SK, SL, from the sun, inclosing a certain space, (when I say "inclosing a certain space," I mean inclosing a superficial area, containing a certain number of acres, or of square miles,) and if in another part of the orbit I draw two lines S*k*, S*l*, and if the two lines S*k*, S*l*, inclose the same number of acres as are inclosed by SK, SL, then the planet will be just as long moving over the long arc KL, as in moving over the short arc *kl*. From that law of equal areas in equal times, you will see that the planet is moving much more rapidly between K and L, than between *k* and *l*.

Having spoken so much of the motion in an ellipse, I will now proceed to speak of the cause of that motion; the force of the sun's attraction, which acts upon every planet. We are now coming to a thing totally different from what we have had before. We have spoken of the form of the orbits of planets, and the proportion of their speed in the different parts of their orbits, as determined from the observation of the planets; and I will now proceed to the consideration of the causes of these motions, in reference to the mechanical theory, first propounded by Sir Isaac Newton, and received by every person who possesses a competent acquaintance with the subject. The theory is this: that if we suppose the planets to be once set in motion, (by some cause which we do not pretend to know,) then the attraction of the sun

accounts for the curved form of their orbits, and for all their motions in those orbits. Now, in speaking of this, I must observe that there is a term frequently used by persons not acquainted with its real meaning; persons speak of "projectile force" as if such a thing was constantly in action. The planets are in motion; they have been put in motion somehow; but there is no force to maintain their motion afterwards, that we know of, and there is no necessity for us to suppose the existence of a force which keeps up that motion. But, having been once started with a certain velocity, it is necessary to suppose that there is a force constantly pulling them towards the sun. The planets will sometimes go away from the sun, the sun will pull them back, and afterwards they will go away again, and be again pulled back, and so on.

In order to explain this, I must proceed with a very rude experiment on what is called the second law of motion. The first law of motion is simply this: if a body be once set in motion, and if it have a certain velocity given to it, it will continue to move (if not acted on by another force) in a straight line with unabated velocity. We cannot make experiments proving this law in the simple form in which I have mentioned it, but we can make experiments on it in combination with other laws; and we are compelled to believe that the law is true, that if a body were started in motion, and if nothing were acting upon it, it would continue in the same motion. The second law of motion is that which may be illustrated by a very rough experiment. Suppose a body to be projected horizontally, like a cannon ball, or like a stone thrown horizontally, you will observe that it begins to curve in its path, under the attraction of gravity, and it falls to the ground. The

second law of motion is, that the force of gravity draws it just as far from the place which it would have reached if no gravity were acting, as the force of gravity would draw it in the same time from the position of rest. Suppose, for instance, that a body is thrown in the direction of AB, (Figure 31,)

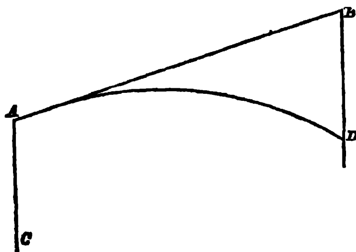


FIG. 31.

with a speed which would have carried it from A to B in one second of time; and suppose I know from experiment that it could have dropped from A to C in one second of time; then the second law of motion is this: that at the end of one second of time the body will really be found at D, having, by the action of gravity, been pulled away from the place B, which it would have reached with the original direction and the original velocity, just as much as if pulled away from the state of rest.

The law may be illustrated by experiments in this manner: AB, Figure 32, is a board; CD an arm moving upon it, turning on a hinge at C, and driven by a spring E; at the end D of the arm is a hollow, with its opening in the side of the arm large enough to contain a small ball, so that when the arm is driven by the spring E, the ball will be thrown

horizontally from the hollow at D; at F is another chamber opening downwards, its lower opening being

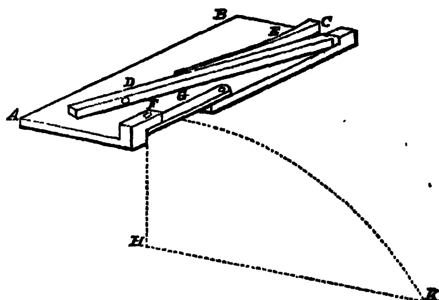


Fig. 32.

stopped by a board G, which will be knocked away by a blow of the arm CD; then it is plain that if one ball be put in D and another in E, the very same movement which throws one ball forward causes the other ball to drop at the same instant; and if the second law of motion be true, one of them will fall down vertically to the floor at H at the same instant at which the other, which is projected forward, reaches the floor at K. And this does really happen so; the two balls do reach the floor at the same instant. What it proves is this: that if a ball is thrown horizontally, it falls from that horizontal line down to the ground just in the same period of time as a ball which dropped from a state of rest.

I have described this experiment as applicable only to a horizontal throw; but it is equally applicable to an inclined throw, if the floor upon which the balls fall be inclined exactly in the same degree,

as is shown in Figure 33, the ball which drops down to H, and the ball which is thrown in the inclined

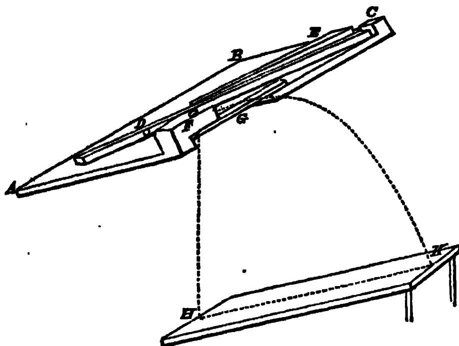


FIG. 33.

direction and reaches the floor at K, will arrive at the floor at the same time.

Now, it is important to observe what are the circumstances on which the curvature of the path of a projected ball depends. In the first place, if anything were to increase the force of gravity, the track of the ball would be more curved. Thus, in Figure

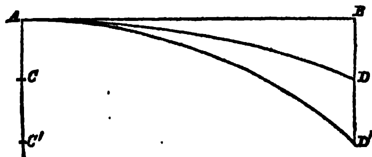


FIG. 34.

34, if the velocity with which the ball is projected

would carry it in a second of time to B, and if gravity were so strong that in one second the body would fall from rest to C, then the ball would describe the curve AD ; but if gravity were so much increased that a ball would fall from rest to C′ in one second of time, then the ball would describe the curve AD′, which is more curved than AD. In the next place, if two balls are projected with different velocities, without any alteration in the force of gravity, the path of that ball which is projected with the smaller velocity will be more curved. Thus, in Figure 35, if the force of gravity were such as

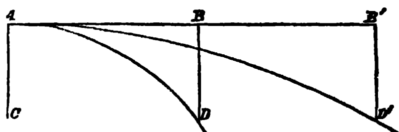


FIG. 35.

would make a ball fall from rest to C in one second, and if two balls are projected, one with a smaller velocity which would carry it to B in one second, and the other with a greater velocity which would carry it to B′ in one second, then the former ball (or that projected with the smaller velocity) will reach D in one second, describing a very curved path AD ; while the latter (or that projected with the greater velocity) will reach D′, describing the path AD′, which is much more nearly straight than AD. Everybody knows the motion of a stone thrown from the hand ; its path is much curved, and it reaches the ground before it has gone far. But if you watch the motion of a cannon ball, which you may do if you stand

behind a cannon when it is fired, as you can then see the ball from the time that it leaves the cannon's mouth to a distance of half a mile or more, you will perceive that its path is curved, but very much less curved than the path of a stone; in fact it is nearly straight, but still not quite straight. The ball dropped downwards through the same space as the stone in one second of time; but the ball travelled further in the horizontal direction than a stone in a second of time. Now, from this consideration, we shall be able to explain something of the most puzzling matters in the motion of planets.

First, however, we must proceed to another consideration, which is called the resolution of forces. This may be illustrated by a model, represented in Figure 36. Suppose A and B to be two pullies fixed

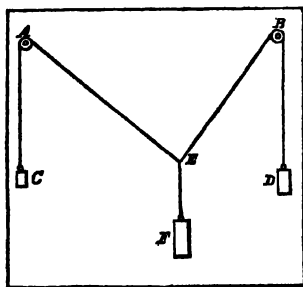


FIG. 36.

upon an upright frame, and suppose two cords to pass over them, carrying the two weights C and D at their ends; and where they meet at E let a third cord be attached, carrying the weight F; then you will see

that the tension or pull produced by this one weight F, acting at the place E, does really support two tensions in different directions acting at the same point, namely, the tension produced by the weight C acting in the direction EA, and the tension produced by the weight D acting in the direction EB. Thus we may correctly say, that one pull in the direction EF does exert two pulls in different directions, AE and BE, for it really does keep the two cords strained to such a degree as to support the two other weights. We may say, on the other hand, that these two outside weights C and D support the middle one. The three pulls of the cords keep the point E in equilibrium; but they will support it only in one determinate position, according to the amount of weight which is hung to each cord. If I put another weight upon C, the position of the point E and the direction of the cords will immediately change; showing that for one proportion of the weights or tensions there is only one set of angles between the different directions at which the tensions will keep the point E at rest; and, conversely, one set of angles of inclination require the tensions to be in one certain proportion, in order that E may be kept at rest. Regarding the action of the two tensions in the directions EA, EB, as supporting the one tension in the direction EF, this may be considered as an instance of the *combination* of forces; and regarding the one tension in the direction EF as supporting two in the directions AE, EB, this may be considered as an instance of the *resolution* of forces, the one force in the direction EF being *resolved* into two forces in the directions AE, BE, and producing in all respects the same effects as two forces in the directions AE, BE. It may seem strange that a force acting in one direction can

produce two forces acting in two different directions; yet we have plenty of familiar examples of the same thing. For instance, in driving a wedge by means of one force, we produce two forces in different directions. I mention that as a case which must be notorious to everybody, and one which may well furnish food for thinking. By pushing at the back of the wedge with a small force you do exert two great forces at the sides of the wedge; and in like manner, by pulling at E (Figure 36) in a downward direction, you may exert a force even greater than your downward pull at the two inclined directions; and both these are accurate instances of what is called the resolution of forces. It must be understood, therefore, that having got a force in any one direction, we may say that instead of one force we have two forces acting in any two directions suited to the nature of the case, whose magnitudes are determined by certain laws depending upon the angles of inclination; and we may use those two forces instead of the one force which we had originally.

From this consideration, in combination with the considerations which I stated relative to the dependence of curvature of path upon velocity and deflecting force, I shall endeavour to give you a little idea of the motion of a planet in its orbit. The thing that I wish specially to explain to you is, how it happens that when a planet has once begun to approach to the sun, it does not go quite to the sun, but after a time recedes again from it. If you understand this, you will understand the rest. I will suppose, if you please, that in Figure 30 a planet is moving from *l*, through M, towards L. The attraction of the sun pulls it in the direction of the line MS. Upon the principle of the resolution of forces, of which I have

just spoken, we may consider the force in the direction of MS to be resolved into two, one of which is in the direction of NM, perpendicular to the orbit, and the other is in the direction of OM, parallel to that part of the orbit. Now, observe this carefully. That part of the force which is in the direction NM, perpendicular to the orbit, produces an effect similar to that which gravity produces in the motion of a cannon ball; it makes the orbit curved. But that part which acts in the direction of OM, parallel to the orbit, produces a different effect; it accelerates the planet's motion in its orbit. Thus, in going from *l* towards L, the planet is made to go quicker and quicker. If you suppose the diagram (Figure 30) turned in such a manner that MS is vertical, S being downwards, you will see that the planet is under the same circumstances as a ball rolling down a hill. If a ball is going down a hill, as at M, Figure 37, the force of gravity, which is in the direction MS, may be resolved into two parts, one of which is a force in the direction NM, perpendicular to the hill side, and merely presses the ball towards the hill; the other is a force in the direction OM, making it to go the faster down the hill. In this manner, as long as the planet goes from *k* through M towards K, it is going quicker and quicker. This accounts for the difference of speed in different parts of the orbit, which I mentioned before. Now, remember how it has been explained that the curvature of a planet's

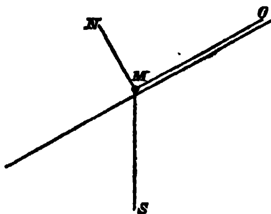


FIG. 37.

orbit, or the curvature of the path of a cannon ball, depends upon two circumstances; one is the velocity with which it is going, and the other is the force which acts in such a direction as to bend its path. The greater its speed, the less its path is curved; consequently, as the planet is going so exceedingly quick in the neighbourhood of K, its orbit may be very little curved there, even though the sun is there pulling it with a very great force. The effect of the planet's path being so little curved there is, that the planet passes the sun and begins to recede from it. But it does not recede perpetually. Suppose, for instance, that it has reached the point M′, and we examine the nature of the forces which act on it there. The force of the sun in the direction M′S may be resolved into two, in the directions N′M′, O′M′, of which the former only curves the orbit, while the latter retards the planet in its movement in the orbit. Therefore, as the planet recedes from the sun, it goes more and more slowly, till at last its velocity may be diminished so much, that the power of the sun, reduced as it is there, is enabled to bring it back again. That is the way the planet goes, revolving in its orbit, alternately approaching to, and receding from, the sun. Of course, in a series of lectures like these, I cannot go into every detail; but enough has been said to show that a planet when approaching the sun will not necessarily fall to the sun, and when receding from the sun will not recede beyond the hope of return. This is a stumbling-block to many a young astronomer who has not considered the subject well, but the remarks I have here made will, on consideration, be found to be perfectly clear, and there is no doubt of their application.

From what I have shown, you will see that there

is a tendency in the planet to go off again when it has come nearest to the sun; but whether it will absolutely go off again depends upon another circumstance: it depends upon the amount of force when the planet is nearest to the sun; as, though the speed be great, it may happen that the force is very great also, and it may happen that the force is so great, that after all we cannot, merely upon general considerations, answer for its coming back again. I wish to point out the general explanation, but it is quite impossible here to enter fully into these particular details, and to show to you whether, when the planet is coming near to the sun, the force will not be too great to allow it to recede again; or whether, when it is going away from the sun, the force will be great enough to bridle it in or not. That is a thing for which you must trust me for a moment. If, as we assume in the law of gravitation, when the distance of the planet from the sun is doubled or trebled, the force of the sun is reduced to one-fourth or to one-ninth, and so on; if that be the law of force, then the velocity of the planet, on coming near to the sun, is so increased that the tendency to recede increases in a greater proportion than the force, and it is certain that the body will begin to recede. But this would not be the case with all laws of force; if we supposed that when the distance was doubled the force was one-sixteenth instead of one-fourth, this law of alternate recess and approach would not be true. Mathematical investigations are made to ascertain whether certain conditions are fulfilled. A planet is invariably moving quickest at that part of its orbit where it is nearest to the sun, and in consequence of that increased velocity of motion, it is able to overcome a degree of attractive force which it would not

overcome if its velocity were not so increased. Under certain circumstances it would go out and come back again, and so on; that is the case with regard to the law of gravitation.

I will now take a few minutes only for the next section. We will depart from the consideration of mechanical forces and consider the measure of distances. The thing which I wish to explain to you is, how we can measure the distance of the moon from the earth. The distance of the moon is measured by the method of Parallax. This is a technical word of which we are obliged to make perpetual use in Astronomy. I will explain in as few words and in as familiar a manner as I can what parallax is. There is an experiment pleasing and profitable, and which I have made in my youth, and which I have no doubt most of you have made in your time. It is this: if you place your head in the corner of a room, or on a high-backed chair, and if you close one eye and allow another person to put a lighted candle upon a table, and if you then try to snuff your candle with one eye shut, you will find that you cannot do it; in all probability you will fail nine times out of ten. You will hold the snuffers too near or too distant; you cannot form any estimation of the distance. But if you open the other eye the charm is broken; or if, without opening your other eye, you move your head sensibly, you are enabled to judge of the distance. I will not speak of the effect of motion of the head at present, but will call your attention to the circumstance, that when your head is perfectly still you will be unable, with a single eye, to judge with accuracy of the distance of the candle. In Figure 38, let A and B be the two eyes, C an object which is viewed first with the eye A only. This eye alone

has no means of estimating the distance of C. All that it can tell is, that it is in the direction of the

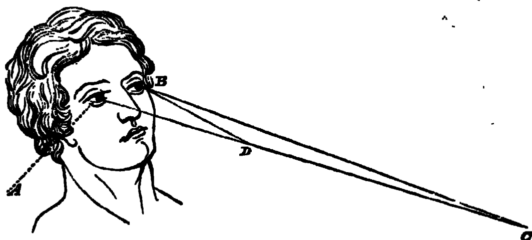


FIG. 38.

line AC; but there is no phenomenon of vision by which it can judge accurately of its distance in that line AC. Suppose, now, the other eye B is turned to C, then there is a circumstance introduced which is affected by the distance, namely, the difference of direction of the two eyes. While the object is at C, the two eyes are turned very little inwards to see it; but if the object is brought very close, as for instance to D, then the two eyes have to be turned considerably inwards to see it; and from that effort of turning the eye, we acquire some notion of the distance. We cannot lay down any accurate rule for the estimation of the distance; but we see clearly enough in this explanation, and we feel distinctly enough when we make the experiment, that the estimation of distance does depend upon this difference of direction of the eyes. When the object is brought very near, the feeling becomes very annoying. This is the principle upon which is founded this experiment of which I have spoken. Now, this difference of direction of the two eyes is a veritable parallax; and this

is what we mean by *parallax*, that it is the difference of direction of an object as seen in two different places. The two different places in the experiment which I have illustrated, are the two eyes in the head. This is the way in which, as will be seen, the distance of the moon from the earth is to be found. The two eyes in the head will be two observatories; and will be supposed to be placed at a considerable distance from each other on the earth. Without any exception at all, the principle is precisely the same. You will thus see how, by the observation of the difference of directions, the distance of the moon from the earth may be obtained. I shall give a more detailed explanation of this in the course of my next lecture.

LECTURE IV.

Recapitulation of Lecture III.—General notions on Parallax.—Method of finding the Moon's Parallax and Distance.—Methods of finding the Sun's Parallax by Transits of Venus across the Sun's Disc.—Causes of failure of other methods.

IN the course of the present lecture I shall depart completely from that part of astronomical observation with which we have been engaged for some time, relating to the apparent motion of the heavens; and in leaving this part of the subject, I shall remark, as I have done before, that there is nothing of so much importance as that you should know the stars and the apparent diurnal motion of the heavenly bodies yourselves; and for this purpose the thing that you should have is a celestial globe. Every person who wishes to know anything of Astronomy should become acquainted with the principal constellations, so as to be able to recognize them at sight in the heavens; to observe their diurnal motion, and the difference in the appearances of the stars at different seasons of the year. This is of the greatest importance, in order to give you an idea of the apparent motion of the sun and planets among the stars.

There are two particular subjects which I have omitted to mention so fully as I would have wished, but I will now allude to them finally. The first is this. In treating of the apparent motion of the sun among the stars, I pointed out to you that it appears to describe a path through the stars which is inclined

to the axis round which the heavens appear to turn; but I said that it is a great circle; which amounts to the same as saying that the path is in a plane which passes through the centre of the globe; or, to express it otherwise, the path is in a plane which passes through the eye of the spectator. If we take a pair of compasses, and open them so that one leg is square to the other, or (as we usually express it) that their angle of inclination is 90 degrees, and if we hold one leg fixed in position, and make it serve as a spindle round which the compasses are to be turned; then the other leg will move in a plane: and if the eye of an observer be placed at the angle of the compasses, the plane which is described by that moving leg will be seen by him as a great circle of the celestial sphere; while by looking along the fixed leg he will see a point in the sphere which is the Pole of that great circle: (the word "Pole" being here used in a general sense, as related to any great circle). From this it appears that the angular distance of the Pole of a great circle, from any point of the great circle, is 90 degrees. If then we suppose a circle to be traced through the heavens, of which every point is 90 degrees from the North Pole, that circle will be a great circle, or will be in a plane passing through the eye of the spectator. That great circle is the equator. Now, as I mentioned in the second lecture, if we trace (by the use of the transit instrument and mural circle) the annual path of the sun through the stars, (which is called the ecliptic), we find that the ecliptic is also a great circle; but it is not the same great circle as the equator. It is inclined to the equator, and crosses it at two points, which are called the first point of Aries and the first point of Libra. The first point of Aries is that crossing of the equator at which

the sun is seen at the Spring Equinox. The first point of Libra is that point at which the sun is seen at the Autumnal Equinox. Neither of these points is exactly marked by any star or other mark in the heavens: the first point of Aries is not very far from the third star of Pegasus, (Algenib) and the first point of Libra is not far from the bright star in the tail of Leo, towards the bright star of Virgo.

The importance of acquiring a knowledge of these points is this. I have spoken of the method of using the transit instrument in combination with the use of the clock, which I said was to determine the interval of time between the meridian passage of some known bright star, and the meridian passage of any other object which we see in the heavens. It was plain, therefore, that by using it in this way we can determine the interval between the passage of the object and that of any star or every star; and also that we can determine, with the same precision, the intervals of the passages of all the bright stars; and we must do this, if we wish to make our representation of the heavens and of the position of an object in the heavens at all complete. This, however, would be a most tedious way of doing it. There is no good way of doing what is equivalent to this, except by referring every interval of passage to some one imaginary point or Zero: and the imaginary point or Zero which all Astronomers have found it convenient to adopt, is the first point of Aries. It is a point, as I have said, which we cannot see in the heavens, but which we can determine by tracing the motion of the sun among the stars. We must observe, by means of the mural circle, on what day or between what days the sun is 90 degrees from the Pole; then the sun is necessarily at the place where his path crosses

the equator, (which, as I have said, is 90 degrees from the Pole,) and therefore is necessarily either at the first point of Aries or the first point of Libra. We must, at the same time, by means of the transit instrument, determine the intervals between the passages of the sun, and several fixed stars on those days; and then we shall have the interval between the passages of the first point of Aries and those stars. We can then use the first point of Aries, so determined, as a starting point for sidereal time; and then instead of measuring our sidereal time from the passage of any star, we shall measure it from the passage of that imaginary point. Now, suppose that our observations of transits on any evening are to be compared with the observation of the transit of the bright star of Aquila; even though we do take that bright star of Aquila as the practical starting point of our observation, yet we do not make our clock to point 0 hours, 0 minutes, and 0 seconds, when the star comes to the meridian, but we put our clock to point 19 hours, 43 minutes, and some seconds; because if we put our clock to point 0 hours, 0 minutes, and 0 seconds, when the first point of Aries is passing the meridian, it shows 19 hours, 43 minutes, and some seconds, when the bright star of Aquila passes the meridian. And that is the use of the first point of Aries, which cannot be seen in the heavens. It is better that an imaginary Zero be chosen for the starting point than any one star; and the first point of Aries is peculiarly convenient, on account of its relation to the sun's path.

The next point of which I omitted to speak is, the difference between a sidereal day and a solar day. I pointed out to you how, by very rough means, the passage of the sun through the stars might be

observed. I said that it might be observed by any person, if he watched, at a given hour of the night, the appearance of the stars on successive days and months; when he would find, on going from one month to the next in the order of succession, taking always the same hour of the night, that the stars appear to go round towards the right, or towards the west, (our faces being turned towards the south); which, as I explained, proves that the sun appears to go through the stars towards the left or the east. From this it is plain that the stars set a little earlier every day, in reference to sun-time; or, that they pass the meridian a little earlier every day in reference to sun-time; and therefore, if we define the sidereal day to be the time that elapses from the passage of a star over the meridian one day to the passage of the same star another day, that interval or the sidereal day will be less than a solar day. It is, in fact, about 23 hours, 56 minutes, and 4 seconds of ordinary clock time: the mean solar day being 24 hours. To sum it up in a few words, the stars appear to be going every day in their diurnal motion from east to west, and they appear to be passing the meridian quicker than the sun does. The sun appears to be travelling from west to east among the stars; and therefore, though the apparent diurnal motion of the sun through the heavens from east to west is quick, yet, in consequence of this apparent motion through the stars in the opposite direction, it passes the meridian slower than the stars do.

In speaking of surveys, I explained that by the use of the transit instrument and the theodolite, we might ascertain the angle made by any one side of a triangle with the meridian. This can of course be done at as many stations as we please; it is commonly

done at two or three. In the English language we have no term for expressing that peculiar act of determining the direction of a side of a triangle, or the direction of a chain of triangles, and therefore we have adopted a word from the French, "orientation"; it is, however, a bad word, used only for the want of a better word in the English language. Where the word "orientation" is used, it is understood to mean the ascertaining the general direction of a chain of triangles.

In regard to the use of the Zenith Sector, of which I have spoken so frequently, I should wish you to charge your memory with this one notion: when that instrument is used for determining the measure of a degree of the earth, by being transported to two different stations, as A and B, Figure 18, and by being employed for observing the same chosen star at both places, the direction of the telescope is really the same at the two places, but the direction of the plumb-line is different at the two places. But, if we consider it only as a matter of observation at each of the places, then we fancy that the direction of the plumb-line is apparently the same in the two places, and that the direction of the telescope is apparently different. Thus the direction of the telescope, when pointed to the same star, is apparently different at Shanklin from what it is at Balta; but, in point of fact, the direction of the telescope is the same at both, and the direction of the plumb-line, or the direction in which a stone would fall, is different at the two places.

In yesterday's lecture I entered upon the subject of the motion of the planets; and I endeavoured to give you a notion of the complexity of the phenomena of the planets, and of the system first used to explain

them. I remarked that for the inferior planets, this was made easier by referring their apparent motion to the sun. I pointed out that Venus moved (apparently) sometimes to the left of the sun, and sometimes to the right of the sun; never exceeding a certain angular limit to the left or right of the sun. I told you that her motion was sometimes backward and sometimes forward; and when referred to the sun, was pretty nearly similar, first on one side of the sun and then on the other side of the sun. All those conclusions are obtained by determining the places of Venus and the sun, with respect to the stars, by means of the transit instrument and mural circle, in the way described in my first lecture, and then laying them down on a globe, or treating them by other methods of calculation. There is no difficulty at all in figuring to ourselves, as the ancients supposed, that this apparent motion of Venus may be generally represented by supposing that she describes a circle round some imaginary centre, which centre is always in the line joining the earth and sun; and that the earth is at no very great distance outside that circle: as, for instance, in Figure 26, if we suppose Venus to revolve in the circle V, whose centre *v* is in the line ES. They had similar notions in regard to Mercury, the centre of whose orbit might be supposed to be in some other place, as *m*.

With regard to Mars, Jupiter, and Saturn, it was supposed by the ancients that they also moved in circles, and that the centres were somewhere in the same line ES*mn*, but that the circles were so large that they completely surrounded the earth. If we consider the apparent motion of Jupiter at the present time, when he is seen nearly opposite to the sun,

rising nearly at sunset, and setting nearly at sunrise, and if we watch his course among the stars from day to day, or if we determine his place on different days by observations with the transit instrument and mural circle, we find that at this time Jupiter is moving among the stars towards the right hand.

It is convenient to have astronomical terms to describe this direction, without speaking of the right hand or left hand. Now, I have explained to you that the appearance of the stars, at different months of the year, shows that the sun must be supposed to move through the stars towards the left; and the moon moves visibly towards the left. Astronomers therefore have agreed to describe this kind of motion by the term "direct," and the opposite motion by the term "retrograde." Now the planets sometimes move in a retrograde direction: thus, when Mercury or Venus is in that part of its orbit which is nearest to the earth, its motion, as referred to the stars, is retrograde. And the apparent motion of Jupiter at the present time, from the description of it which I have just given, is retrograde; and so in all cases is that of Mars, Jupiter, Saturn, Uranus, Neptune, and the smaller planets, when they are seen on the side opposite to the sun. At other times their apparent motions are direct with respect to the stars.

All these motions are tolerably explained by the construction adopted by Ptolemy and the Greek astronomers: taking the assumption that the earth was fixed; that there was something like a bar, (Figure 26,) of which one end was fixed in the earth, and which turned round in a year; that that bar carried the sun, and carried also the centres of the orbits of Mercury, Venus, Mars, Jupiter, and all the planets; and that all the planets revolved in their

own orbits round their respective centres; the planets Mars, Jupiter, and Saturn, being supposed to have a retrograde motion with respect to the bar. I also said that it was found necessary, in order to account for the motion a little more accurately, to suppose that the planets did not revolve strictly in circles, but that the radial bar as *nMa*, Figure 26, carried another bar as M*a*N, jointed on it, and moving on the joint, and that this second bar carried the planet. Supposing a similar construction of each of the planets, we get a terrible complexity of motions, and all independent of the sun.

I then remarked, it was a strange thing that persons did not think of connecting these motions more closely with the sun. It would have answered their purpose quite as well to take one centre as another centre for the orbits of the planets, provided it were in the same direction, and provided the proper dimension were given to the orbit: that, for instance, in Figure 27, the apparent motion of Venus, as seen from the earth E, would be the same, whether it moved in the small orbit V, whose centre is *v*, or in the large orbit V′, whose centre is S. Having, then, the power of choosing the centre of the orbit as we please, we might as well take the sun for the place of the centre; and it is wonderful to me that such a simplification was not sooner adopted by the ancient astronomers.

The system Copernicus fixed upon in his successive steps was, first to bring all the centres of the orbits to the sun—still retaining the notion that this sun, together with the various orbits connected with it, were carried round the fixed earth—and then to suppose that the earth was in motion round the sun, (which would explain the appearances just as well,)

or that the earth, as well as the other planets, moved round the sun. With this supposition, the motions of Mars, Jupiter, and Saturn are direct, but slower than that of the earth. And that was the state to which Copernicus reduced it; but still Copernicus was not freed from the notion that the small bars attached to the large ones carried the planets, as I have described. Kepler, a man who never spared his labour in working out any theory, after an infinity of trials, at last found out that he could represent everything perfectly well, by supposing that everyone of the planets moves in an ellipse, of which the sun is the focus; that the orbits of the different planets have different degrees of ellipticity; that the long axis of the ellipse is in different positions for each different orbit: of course it required an infinity of trouble to work that out. He established what is called Kepler's first law—that each of the planets revolves in an ellipse, of which the sun is one focus.

I will mention at once the third law which Kepler established, and which relates to the proportion of the periodic times of the different planets, and the proportion of their distances from the sun. Knowing the proportion of the distances of the planets from the sun, and knowing the periodic times of the planets round the sun, he was able to work out this rule. If we square the number of the days in the time of each of the planets going round the sun, we shall find that the squares of the times of revolution of the different planets are in the same proportion as the cubes of their mean distances from the sun. That was a most important thing to establish.

The second of Kepler's laws was this. In Figure 30, let S be the place of the sun in the focus of a planet's orbit; suppose that in one day the planet

goes from K to L, and that in another part of the orbit it goes in one day from *k* to *l*; the law which Kepler made out was this: taking the areas which are included by the lines drawn from the extremities of these arcs straight to the sun; then the area KSL is equal to the area *k* S *l*. You will observe from this law, that it is quite evident that each planet moves quicker in that part of its orbit which is nearest to the sun than in that part of the orbit which is more distant from the sun; because the whole area described in one day or in a certain number of days is the same in the two cases. This was ascertained from observations, and without any notion of mechanical theory; Kepler did not possess any notion of that kind.

There is only one more matter which I will mention before I proceed to the mechanical consideration of the subject. Without knowing the distance of the earth from the sun, and without knowing the distances of the planets from the sun, we do know the proportion of their distances. This is because, as a matter of observation, we know how much the planet (Mercury or Venus, suppose) appears to go to the right or to the left of the sun. In Figure 27, it will do just as well to explain the phenomena of the planet Venus, whether we suppose that the sun is at S, or whether we suppose that the sun is at *v*, provided we suppose that the dimensions of the orbit V′ are large, and those at the orbit V are small, in the same proportion as the proportion of the distances of S and *v* from E. For instance, we may make one supposition that the earth is a hundred millions of miles from the sun; and that Venus is seventy-two millions of miles from the sun. Or we may make another supposition—that the earth is only fifty

millions of miles from the sun, and that Venus is only thirty-six millions of miles from the sun. With the latter supposition (in which the distances are in the same proportion as in the former) we should find that Venus will appear to go just as much to the right or to the left of the sun as with the former. And, therefore, when we find that the apparent motions, computed on the supposition that the distances of the earth and Venus from the sun are respectively one hundred and seventy-two millions of miles, do agree with those which are really observed, we cannot tell whether the real distances are one hundred and seventy-two millions of miles, or fifty and thirty-six millions, or any other number; all that we know is that they must be in that proportion. It is important to observe that this was the foundation of the third of Kepler's laws, and that he knew, as well as we do at the present time, what is the proportion of the distance of Venus from the sun to the distance of the earth from the sun, although he had not the slightest knowledge of the absolute distance of the earth from the sun.

I shall now proceed with the mention of the mechanical laws of orbital motion. In the first place, I shall take into consideration the general effects of attraction, or force; in which expression by the word *force* I mean pressure producing an effect on the motion of bodies that are free. Suppose we drop a stone from our hand, or from a high building, everybody knows that it begins to fall with a very small velocity, and that it gains velocity as it falls. If I were to drop a stone from my hand at the height of a foot from the floor, it would fall lightly; but if I were to drop it from the height of a hundred feet, it would fall with a great shock. It is therefore evident that

any falling body is accelerated in its motion. This shows that the effect of gravitation is not to create a sudden velocity, but to add velocity to velocity, and continually to increase velocity. Now, that is a thing which you must consider in regard to the motion both of bodies falling in a straight line and of bodies projected and allowed to fall in a curve.

It will be remembered that I exhibited an experiment to this effect—that if any body were projected horizontally at the same time that another body was allowed to fall freely, they would both reach the ground at the same time. The apparatus, Figure 32, by which the experiment was made, is so constructed that, of necessity, when one body is projected forward the other is allowed to drop at the same time. Now, let us consider what sort of a curve the projected body would describe. Suppose a shot is projected from a cannon, as A in Figure 35; as I said before, if a ball would fall from the cannon's mouth to the point C in a second of time, then the shot which was fired out of the cannon would have dropped to D in one second of time. What sort of course would it have described? It would have fallen from its original direction in exactly the same proportion, so far as regards the divisions of the time, as the ball which dropped from the cannon's mouth. The ball dropping from the cannon's mouth does not acquire all its velocity downwards at once, but by degrees. In like manner, if this other ball is supposed to be thrown out horizontally towards B, it does not begin to drop suddenly, but drops more and more rapidly; it follows, therefore, that the path of the cannon-shot begins to turn more and more downwards, and assumes the form of the curve which is shown in the figure. You see that the form differs very little from

the circle, and it is what mathematicians call a parabola.

The same consideration is applied to the motion of a planet, in this manner. We must suppose that the planet has been put in motion; we cannot tell how the planets have been put in motion, but they are in motion; that is sufficient for our purpose. The planets, if there were nothing to pull them aside, would go on in a straight course, without altering their velocity. The supposition which Newton made, and on which is founded the theory of gravitation, and which is perfectly conformable with every result of observation, is, that all the planets are attracted towards the sun; that the force is different in different parts, but still always directed towards the sun; that the force is of such a character that it is greater the nearer they go to the sun. Thus, if a planet started from P, Figure 30, in the direction PQ, it would go on in a straight line if it was not pulled by the attraction of the sun; but, by the attraction of the sun, the orbit becomes bent, and the planet describes the curved orbit P*kl*MKL. Now, though this reasoning shows most clearly that the planet will move in a curved orbit of some kind, it is entirely impossible for me in this oral lecture to tell you how the precise nature of this curved orbit is found out; it is, however, found out completely; and I must beg of non-mathematicians to take my word for the result. When the investigation is conducted thoroughly we obtain these results. First, taking for granted Kepler's second law "that each planet, considered without reference to other planets, does in equal times describe equal areas by the line connecting it with the sun," which law was ascertained purely from observation: it is found that this is

explained by supposing that the planet is at all times drawn by some force toward the sun. Secondly, supposing a planet put in motion, and then continually acted upon by any force whatever, directed always to the sun, it is found that it will describe equal areas in equal times by the line connecting it with the sun. Thirdly, taking for granted Kepler's first law, which was ascertained from observation only, "that the planets in their revolutions describe ellipses," we can ascertain what is the force with which they are drawn towards the sun, and which causes them to describe ellipses; it is found to be an attraction towards the sun following the law of the inverse square of the distance; that is to say, when the planet is half-way distant from the sun, it will be drawn with four times the strength, or, when the planet is one third distant from the sun, it will be drawn with nine times the strength. Fourthly, we may propose to ourselves this problem: suppose the planet to be once put in motion, and then continually attracted by the sun with a force inversely as the square of the distance from the sun, what curve will it describe? It is found, by the investigation which I have spoken of, that the curve which it will describe will be one or other of the following—a circle, an ellipse, a parabola, or a hyperbola; and that the sun will be in the centre of the circle, or in the focus of the ellipse, parabola, or hyperbola. In nature we do not know any instance of the hyperbola; comets, as we shall see hereafter, for the most part move in parabolas; some comets and all the planets move in ellipses; and some of these ellipses approach very nearly to circles. I am exceedingly sorry that it is impossible for me to give you an idea of the steps of these investigations; but I say, and I am sure you

will agree with me, that half a man's life would be well spent in mastering them.

Kepler's third law is this: that if we compare the orbits of the different planets, the squares of the periodic times are in the same proportion as the cubes of the mean distances from the sun. This also, in conformity with the result of the theory of attraction following the law of the inverse square of the distance. I suppose there is no science in the world in which such important laws have been first discovered independently from observation only, and which have afterwards been shown to be the result of one grand principle of theory.

I next endeavoured to point out to you how it is that planets do not entirely depart from the sun. It has been wondered by some persons that, when the planets approach to the sun, they are not compelled by its attractive force to fall into the sun; and when they go away from the sun, it has been wondered that they do not go quite away from the sun's influence. I endeavoured to give you a notion of the mechanical causes which produced that alteration in the velocity of the planets, which is in fact embodied in that law of Kepler's, which states that a planet describes, by the lines connecting it with the sun, equal areas in equal times. In introducing that matter, I said, that the curvature of the orbit of a planet, in the same manner as a curvature of the path of a cannon ball, depends not only upon the force which pulls the planet or the ball so as to curve its orbit, but also upon the amount of velocity with which it is moving. Therefore, in order to ascertain what is the curvature of an orbit, we must consider not only the amount of the sun's attraction at any part of it, but also what is the amount of velocity with which the

planet is moving at that point. If, then, it can be shown that when the planet is nearest to the sun it moves with a greater velocity, it will follow that, though the attractive force is greater than when farthest from the sun, its orbit may not be more curved than when it is farthest from the sun.

For this purpose, I introduced to you the model, represented in figure 36. I pointed out to you that the tension of the cord EF, acting in the direction EF, does produce the effect of keeping in equilibrium the tension of the two other cords, one acting in the direction of EA, and the other in the direction EB; and therefore, a force acting in the direction EF does produce two forces acting in the directions AE, BE. This is what we mean by the resolution of forces. The way in which I desire to apply this consideration to the motion of the planets is this. The sun's attraction acting in the direction MS, Figure 30, can there be resolved into two forces, in conformity with the law just mentioned; one in the direction of the line OM, touching the orbit, or in the same direction as the motion of the planet, and the other force in the direction NM, perpendicular to the orbit.. As regards this part of the force which is perpendicular to the orbit, its effect may be considered as similar to that of the force of gravity on the cannon ball; its action is square to the planet's path at that time, and its tendency is to curve the planet's path. But the other resolved part pushes the body along in its orbit, so that the planet, instead of being allowed to go on in one uniform speed, is by the sun's attraction accelerated in its course. This amounts to so great a quantity that, at the time the planet has arrived at the part L of its orbit, it is going at a very great speed. When the planet has arrived at the part of the orbit

where the force is so great, it has been accelerated so much that its velocity also is very great. If the body had not been moving quicker, its path would have been much curved; but in consequence of its great velocity, its path may be very little curved; the power of the sun may be unable to bridle it any longer, and it may go on from that point increasing its distance from the sun. When it has thus reached a point as M′, if we resolve the sun's force into two forces, one perpendicular to the orbit, and one in the direction of the orbit, then that which is perpendicular to the orbit bends it, but that which is in the direction of the orbit retards it; and when it has got to a certain distance, its velocity is small indeed; and though it is so far off that the sun's force is very small, nevertheless, in consequence of the planet's diminished speed, the attractive power of the sun may be able to pull it in and make it describe the same orbit again; and thus the planet need not either fall into the sun when nearest, or go quite away when farthest.

There is another thing which I think it very proper to mention, because many persons have a very erroneous notion upon it. Some persons have a notion that there is some remarkable adjustment, so that if anything however small was to disturb the motion of a planet, it would either fall to the sun or go quite away from the sun. That is not the case; the effect of the disturbance of a planet would be to change its orbit, but nothing else. I will endeavour to point out to you what I mean. Suppose that a planet has been going on describing the orbit LPRM, Figure 39, for ages, continually describing the same curve in an ellipse round the sun. Now, I will suppose that, when it is nearest to the sun at L, something comes

in the way and retards its motion. Many persons suppose that, in consequence, the planet would fall into the sun. No such thing will happen; the only effect it would have is this: it would cause the planet to describe a different orbit, such as is shown by the dotted line L*prm*. Its velocity would be diminished by the interruption at L, and it would consequently be more bridled in by the attraction of the sun there, and the planet would then describe a new orbit of such a nature as to have a greater curvature at L; but if nothing disturbed it again, it would then go on continually describing that new orbit over and over again. Whenever the series of disturbances ceases, whatever orbit the planet is moving in, it will continue to go on moving in that orbit. The planet's orbit is changed by any sudden disturbance, but the orbit so changed will continue, and the planet will be no nearer destruction than if it had not been disturbed at all.

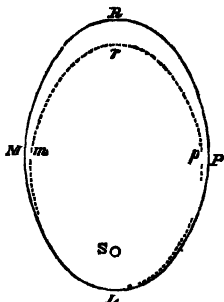


FIG. 39.

There is only one more point regarding the law of gravitation, on which I shall here speak; it is the velocity, or the change of motion, which an attracting body produces on another body. I have spoken of attraction as if it was directed towards the sun; but we shall find that experiments of various kinds lead us to this conclusion: that every particle of matter attracts every other particle of matter; and that every planet attracts every other planet, that every planet attracts the sun, that the sun attracts

the planets, that the sun attracts the moon, and the moon attracts the sun, and that every body attracts every other body. Now, the thing I wish you to understand is this: suppose Venus and the sun are at equal distances from the earth, then the earth pulls the sun out of its way just as much as it pulls Venus out of the way. The enormous difference of magnitude of the attracted bodies makes no difference in the movement which the action of the attracting body produces on them. If there are two bodies, a great one and a little one, and if something else attracts them, the great body is pulled through as many feet or miles in an hour as the little one.

I shall say nothing more about this subject at present, but proceed at once with the measure of the distance of the various heavenly bodies. I endeavoured to give you a notion of what we call parallax; I endeavoured to illustrate it by showing the combined effect of the two eyes in our head. I pointed out to you, by a familiar experiment, that it is not easy to obtain, with a single eye and when the head is held unmoved, a correct notion of the distance, but that if we open both eyes, we then get an accurate knowledge of the distance. This I remarked is exactly similar to the effect of observing the same object at two observatories, which are planted at two parts of the earth at a considerable distance from each other.

Now, in Figure 40, let GC be the earth, M the moon. I wish to measure the distance of the moon from the earth. I have two observatories from which I view this moon. One of them, G, we will suppose to be at Greenwich or at Cambridge; the other, C, to be at the Cape of Good Hope. In remarking on the grounds of a person's judgment of the distance

of an object D, Figure 38, as observed with the two eyes, I said that it depends on this: that the object

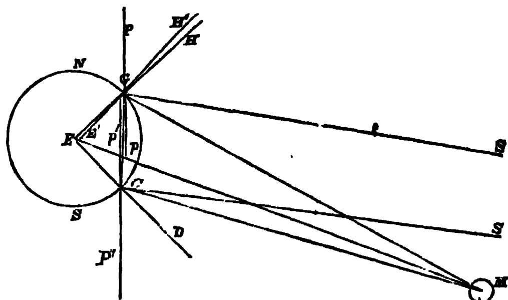


Fig. 40.

is seen by the two eyes in two different directions. In like manner the measure of the distance of the moon, by means of observations made at the two observatories G and C, will be based upon this circumstance: that the moon is seen in two different directions.

How can that difference of direction be ascertained? It can be ascertained by observing, at each of these observatories, the Polar distance of the moon. It will be remembered (see page 33) that, by the use of the mural circle at Cambridge, or at Greenwich, we observe the elevation of the celestial North Pole; and by the use of the same mural circle, we observe the elevation of the moon on the other side of the Zenith; or to use a more convenient measure, which we more frequently employ, we measure the angular distances both of the Pole and of the moon from the Zenith. Thus, suppose that by observations made at G on one side the Pole is 38 degrees from the

Zenith, on the other side the moon is 70 degrees from the Zenith; if we add together these two angles, we shall see that the moon is 108 degrees from the North Pole, as seen at Greenwich, G. This 108 degrees is the measure of the angle PGM, GP being supposed to be directed to the celestial North Pole. At the same time observations are going on at the Observatory C, at the Cape of Good Hope, where they cannot see the North Pole, but they can see the South Pole, and therefore they must refer their observations there to the South Pole; suppose that there they find the angular distance of the moon from the South Pole to be 73½ degrees, this is the measure of the angle P′CM, CP′ being supposed to be directed to the celestial South Pole, and therefore parallel to GP.

Now, we have got these two measures, from which we see, that by the combinations of the Zenith distances at Greenwich, the moon is seen at 108 degrees from the North Pole; and by a similar combination of the Zenith distances at the Cape of Good Hope, the moon is seen 73½ degrees from the South Pole. If we were observing a star S at an immense distance, we should get this relation between the two angular measures; that the sum of the two angular measures, one from the South Pole and the other from the North Pole, must be 180 degrees; inasmuch as the two directions GP and CP′ are exactly opposite, and the two directions GS and CS, on account of the immense distance of a star, are exactly parallel; and therefore, in turning a line first from the position GP to GS, and then from the position GS or CS to CP′, we have turned it exactly half-round. But the thing which we have found out with regard to the moon is this: that the sum of the two angular measures is

more than 180 degrees, for the measure at Greenwich being 108 degrees, and that at the Cape of Good Hope being 73½ degrees, the sum of these is 181½ degrees. How are these 1½ degrees to be accounted for? This angle of 1½ degrees is the angle made by the two lines GM, CM. I have arrived thus at the conclusion: that at the distance of the moon, the angle between the two lines, of which one is directed to the Greenwich Observatory, and the other to the Observatory at the Cape of Good Hope, is 1½ degrees. Having the position of Greenwich and the Cape of Good Hope, knowing the angle is 1½ degrees, and knowing the directions of the lines from the two Observatories, I can compute the distance of the moon at once.

Here we must remark, that the observations of which I have spoken would be of no use, if we had not got the measure of the earth. That measure, however, as I explained in a former lecture, has been obtained by the aid of our yard measure; and it is worth while to recall the principal steps of the process. By means of the yard measure we measured a base line in a survey— by means of the base line, with triangulation, we obtained the length of some very long lines upon the earth's surface; and by the use of the Zenith Sector at different parts of the earth, in combination with the measures of these long lines, we got the general dimensions of the earth as expressed in yards; then, knowing the position of the Observatory at Greenwich, and knowing the position of the Observatory at the Cape of Good Hope, we have the means of getting from these general dimensions of the earth the length of the line GC in yards, and its position in regard to the other lines; we know also that the angle GMC is 1½

degrees, and therefore we have the means of computing the length of GM or CM expressed in reference to our yard measure; that is, of ascertaining how far off the moon is from the earth as expressed by a yard measure.

In the middle of the last century, the celebrated French Astronomer, the Abbé de la Caille, was sent to the Cape of Good Hope to make the requisite observations; observations were also made at the same time at the Observatories at Paris and at Greenwich, to determine the angle spoken of. A few years ago, Mr. Henderson and Mr. Maclear, who were successively sent to the Cape of Good Hope, were partly employed in making observations for the same purpose; and from the observations also made at the same time at Greenwich and at Cambridge, the distance of the moon from the earth has been determined.

I will now mention the only failure likely to take place in consequence of pursuing the method which I have described. It is this: I pointed out to you in the first lecture that a correction for refraction is necessary for every observation made with the mural circle. In consequence of that refraction all objects, in every part of the heavens, appear higher than they really are; a correction is applied to that circumstance, but that correction may be liable to a small uncertainty. The angular distance of the line GM from the line GP, directed to the North Pole of the heavens, may therefore be slightly in error: we can come very near the truth indeed, and perhaps we should not be wrong a single second, or half a second—but still there is always some uncertainty; and I may say, that refraction is the very abomination of astronomers. In like manner there may be a small error in the angle MCP′, and it may happen that the two errors

are combined, so as to affect the determination of the angle GMC by a larger error.

Now there is another way in which this observation can be shaped, in which the effect will not be quite so bad. This is by referring the place of the moon, as seen at each Observatory, not to the North and South Pole as I have spoken, but to a star. Let S, Figure 40, be a star at a very great distance, and suppose we observe the moon when she is nearly in the direction of that star. Now, the thing which it is our object to ascertain, is the angle made by the two lines GM, CM. At G the moon is seen somewhat below the star; we have then only to measure the angle SGM, about which there can be very little uncertainty, either from refraction or from any other cause. At C the moon is seen still more nearly in the same direction as the star; the angle SCM can therefore be measured with great accuracy. The angle GMC is the difference between the two angles SGM, SCM. Suppose, for instance, that at G the moon is seen two degrees below the star, and at C is seen only half a degree below the star, then the difference or the angle GMC must be $1\frac{1}{2}$ degrees; and this angle is scarcely liable to any possible error. We have then got this angle GMC accurately, and we have got the directions which the two lines GM, CM, make in reference to the line GC; and the calculation is then much the same as with a triangle in a survey, where we have a base measured from which we can begin our computations. This is the way in which the distance of the moon is measured; and we may say, as a general result, that the distance of the moon from the earth is about thirty times the breadth of the earth.

It is necessary now to explain with a little more

precision what we mean by the word *parallax*. It is convenient to make all our calculations of the moon's place with reference to the centre of the earth. Now, in Figure 40, it will be seen that the moon, if viewed from the centre of the earth, would be seen in the direction EM; but, as viewed from Greenwich, she is seen in the direction GM. The difference between these two directions is the angle GME, and this is called the moon's parallax at Greenwich. In like manner, the angle EMC is the moon's parallax at the Cape of Good Hope; and therefore the angle GMC, which has been found by observations in the way already described, is the sum of the parallaxes of the moon at Greenwich and the Cape of Good Hope.

Now, the method in which the calculation of the moon's distance is actually effected is this. From a knowledge of the earth's dimensions, the length of the line EG is known with considerable accuracy. And though (as I stated in the second lecture) the plumb-line at G is not directed actually to the earth's centre, but in a slightly different direction, H′GE′, yet, from knowing the form of the earth, we can calculate accurately how much it is inclined to the line HGE, which is directed to the earth's centre. Thus, we know the angle H′GH, and we have observed the angle H′GM with the mural circle, and the difference is the angle HGM, which therefore is known. Then we assume, for trial, a value of the distance EM. With the length EM, the length EG, and the angle HGM, it is easy to calculate the angle GME. The same process is used to calculate the angle CME. We then add these two calculated angles together, and find whether their sum is equal to the angle GMC, which we have found from observation. If the sum is not equal to that quantity found from observation,

we must try another assumption for the length of EM, and go through the calculation again. And this we must do over and over again, till the numbers agree.

It has been supposed that the observations are made at the same instant at Greenwich and the Cape of Good Hope. This is not strictly correct; but the difference of time is known, and the moon's motion is well enough known to enable us to compute how much the angle P'CM changes in that time; and thus we can find what would have been the direction of CM, if the observation had been made at exactly the same instant as the observation at G.

After having got this notion of the value of the moon's distance, and knowing the method of computing the parallax, it is necessary to apply that computed parallax to every observation made, in order to find the position of the moon as seen from the centre of the earth. Now, if we have made a considerable series of observations of the position of the moon as viewed from an Observatory, and then, by calculating every parallax, if we have got the corresponding places of the moon as viewed from the centre of the earth, we find this, that the same law holds in the motion of the moon round the earth as in the motion of the earth and planets round the sun; that is, that the moon moves in an ellipse which is in a plane passing through the earth's centre.

When we consider it sufficiently established that the moon does revolve in a plane passing through the earth's centre, we can take that assumption as the basis of calculation to be compared with observations; and we can find what the moon's distance is, from observations at a single Observatory. The effect of parallax is always to make the object appear lower. If the moon were viewed from the centre of the earth,

its path (which is in a plane passing through the centre of the earth) would appear to be a great circle inclined to the equator; and if we compared its places when nearest to the celestial North Pole, and when furthest from it, one of these angular distances from the celestial North Pole would be as much less than 90 degrees as the other is greater than 90 degrees; and their sum would be 180 degrees. But, in consequence of parallax, each of these angular distances as viewed at Greenwich is increased and therefore their sum is greater than 180 degrees. By ascertaining, therefore, what each of these distances is, and what their sum is, and how much that sum exceeds 180 degrees, we have the sum of two parallaxes; and from this we can find the moon's distance by calculation, (assuming a distance for trial, and altering it as often as may be necessary, and for every alteration computing the two parallaxes, adding them together, and seeing whether their sum agrees with the observed sum), nearly in the same way as when the moon was observed at Greenwich and the Cape of Good Hope. It is remarkable that this principle was used as long ago as by Ptolemy, (about A.D. 130) and a respectable estimation of the proportion of the moon's distance to the earth's diameter was obtained by him; but, as he did not know the dimensions of the earth, he was unable to express the moon's distance by an absolute measure.

I shall now proceed to a subject of much greater difficulty, viz. the computation of the distance of the sun from the earth. This most difficult problem might not have been accurately solved but for a suggestion of Dr. Halley, who, in the year 1716, published a paper in the Philosophical Transactions of

the Royal Society of London. He was an old man at the time, nearly sixty years old. He explained what, as he said, would be a satisfactory method of solution, by observations of the "transit"* (or "passage") of Venus over the sun's disc or face; and he pointed out the possibility of seeing the transits of Venus across the sun's disc, which were to occur in 1761 and 1769; and he bequeathed, as a task to posterity, the problem of ascertaining the distance of the sun from the earth. For, understanding the following statement, it is important to remark, that the method of finding the distance of the sun by observation of the transit of Venus, requires that observations be made (as in the first method for the moon) at two Observatories at widely different positions on the earth. In 1761 a transit of Venus occurred, which was visible in many parts of Europe; it was necessary to observe it in other parts of the earth, and expeditions were sent out for that purpose; amongst others, Dr. Maskelyne was chosen by the Government to go to St. Helena, (where, however, clouds prevented any part of it being seen), and a Mr. Mason to the Cape of Good Hope. In 1769 another transit of Venus occurred, which was visible in the North of Lapland, but in no other parts of Europe; it was necessary, in order to procure a good station for observations to be compared with those in Lapland, to send out an expedition to the Pacific Ocean. Captain Cook was sent out by the British Government to the South Seas, in the year 1769, in order to observe the transit of Venus in the island of Otaheite. I wish to mention with regard to this expedition that, so far as I can understand, the expenses incurred in that part

* The word "transit" signifies nothing more than "passage across."

which related to the ascertaining the distance of the sun from the earth, were defrayed from the private purse of George the Third.

The next transit of Venus will occur in the year 1874. It will be followed by one in 1882: after which there will be none for more than a century.

From the transits of Venus in 1761 and 1769, but especially from the latter, the sun's distance from the earth was ascertained to be about 95½ millions of miles. It was long believed that the determination was all but perfectly accurate: but recent investigations seem to point to the conclusion that the value of the sun's distance so found is too great by more than three millions of miles. The source of error appears to be traced to the untrustworthy nature of the observations made in Lapland: and astronomers now look to the coming transits in 1874 and 1882 for the precise settlement of this important question.

When the distance of the sun is obtained, the distances of the other planets are easily found by calculation from the proportion of distances, which, as I said, was known long before the real distance of any one was known.

Before entering on the explanation of the principles of this method, I will point out to you the nature of other attempts which had been made to ascertain the distance of the sun from the earth, some of which were totally unsuccessful. In the first place, we might use the same method to ascertain the distance of the sun from the earth as that used for ascertaining the distance of the moon from the earth. We might take the angular distance of the sun from the North Pole, as viewed from an Observatory on one part of the earth (as Greenwich); and the angular distance of the sun from the South Pole, as viewed from

another Observatory at another part of the earth (as the Cape of Good Hope); and then, by adding these two angles together, if the sum exceeded 180 degrees, we should consider that excess as due to the parallax, and should calculate the sun's distance in the same manner as in the case of the moon. But then comes in refraction, which is here a serious matter. The sum of the parallaxes of the sun at the two stations, that is to say, the angle GMC, Figure 40, (supposing M now to represent the sun), does not exceed eight or nine seconds; it is an exceedingly small angle. The uncertainty of refraction in the observation of the sun is always two or three seconds; the air being hot at the time the observations are made, and the sun almost always appearing, in a telescope, tremulous and ill defined. It is plain, therefore, that where there is such an uncertainty, it is useless to attempt to determine an angle which is not greater than eight or nine seconds. You may say, perhaps, that we could do the same thing with the sun as with the moon, by referring it to the stars. But there is this difference; we can see the moon, and we can see small stars close to the moon, and at the time that we observe the moon and the stars the air is not disturbed by the heat of the sun; everything is steady and is seen well; but in the heat of the day objects are unsteady and are never seen well: we cannot see a faint star at all; and we can only see bright stars at a distance from the sun; therefore we are cut off entirely from observing the parallax of the sun in that way. We cannot observe its angular distance from the North and South Poles from the uncertainty of refraction, and we cannot compare the sun with the stars, because we cannot see stars near to the sun when the sun is shining.

There is a second method, a very ingenious one, proposed by astronomers in the middle ages. You will observe, by the preceding operations which I have mentioned, that we have made several good steps for the purpose of measuring the distance of the sun from the earth. By a yard measure we have got the length of an arc on the meridian, and from that we have found the dimensions of the earth, and from these we have got the distance of the moon; and possibly, from the distance of the moon from the earth we shall be able to get the distance of the sun from the earth. In Figure 41, let S be the sun, E

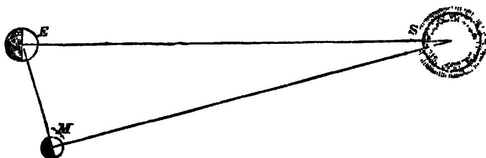


FIG. 41.

the earth, and M the moon, and suppose the distance of the sun to be not particularly greater than that of the moon. The sun illuminates half the moon at once, and no more. In the position which is represented in Figure 41, when the angle SME is a right angle, persons on the earth will see the moon half illuminated, but not at an angle of 90 degrees from the sun. For when the angle SME is 90 degrees, the angle SEM must be less than 90 degrees, inasmuch as the sum of the two angles SEM and ESM is 90 degrees. From this it is plain, as a matter of theory, that when we see the moon half illuminated she is less than 90 degrees from the sun, and the angle at which she really is distant from the sun depends upon

the proportion of her distance to the sun's distance. If the sun's distance from the earth is not excessively greater than the moon's, then that angle SEM will be sensibly less than 90 degrees; but if the sun's distance from the earth is very much greater than the moon's, then that angle will be very nearly equal to 90 degrees. This observation, therefore, gives us, theoretically, the way to determine the sun's distance from the earth, if we can determine exactly the time when the moon is half illuminated. Unfortunately, the roughness of the moon's surface, which resembles very much a volcanic surface, makes it impossible to observe, with any degree of exactness, at what time the moon is half illuminated, and this principle fails totally in practice from that cause.

The next method pointed out for ascertaining the distance of the sun from the earth, and which has been used with considerable success, is founded upon the observation of the planet Mars. Although the distances of the planets from the sun were not known to the ancient astronomers, yet the proportions of their distances were known to them. I have already pointed out to you that Venus is the planet which exemplifies this best. By observing how far Venus goes to the right and to the left of the sun, we can ascertain the proportion of the distance of Venus from the sun to the distance of the earth from the sun, which is a proportion of 72 to 100 nearly; although we do not know the absolute distances at all. The same remark applies to other planets; although we do not know their absolute distances, yet we can see or ascertain the proportions of the diameters of their orbits, which explain their different apparent movements. With regard to Mars, we are able to assert (knowing the proportion of the

distances of Mars and of the earth from the sun, and the form of their orbits) that at certain times, when Mars is in opposition to the sun, the distance of Mars from the earth is only one-third of the distance of the sun from the earth; therefore, if we can ascertain the distance of Mars from the earth at that time, and multiply it by three, we get the distance of the sun from the earth. The distance of Mars can be got at with considerable accuracy, by observations made at Greenwich and the Cape of Good Hope, in exactly the same method as that which I have explained for getting the distance of the moon. Our observations as to the angular distance of Mars from the celestial North Pole must be referred to the same fixed star at the two places. In the Nautical Almanack, for several years past, (a work which is published three or four years in advance,) there is prepared a list of stars, which it is recommended should be observed and compared with Mars, not only at the European Observatories, but also at the Cape of Good Hope and elsewhere in the Southern Hemisphere. Accordingly, at the opposition in 1862, observations were made in pursuance of these recommendations: and from them it has been deduced, that the distance of the sun from the earth is rather more than 91½ millions of miles, a smaller value, it will be observed, than that found from the observations of the last transit. This method is a pretty good one; but it is not the most accurate method, which is that founded upon the transit of Venus, and to the explanation of it I now proceed.

There is one point, with regard to the motion of the planets, to which I should wish to allude in the first place, as I have hitherto made no mention of it. It is rather important in reference to the transit of

Venus. The orbit of each planet is in a plane passing through the sun, but the orbits of all the planets are not in the same plane; the orbit of Venus is not in the same plane as the orbit of the earth. In Figure 42, let S be the sun, E the earth in its orbit,

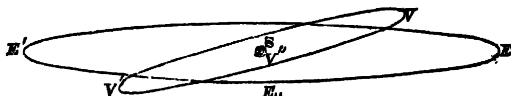


FIG. 42.

and V Venus in her orbit. It is plain that Venus in her motion crosses the plane of the earth's orbit in two positions, and no more. Suppose, now, that E and V represent the relative positions of the earth and Venus, at the time when they are, generally speaking, in the same direction from the sun. If a person on the earth looks at Venus now, he will see her above the sun. If it happen that the two are, generally speaking, in the same direction from the sun at E′ and V′, then when the conjunction takes place she will be seen below the sun. But there is one state of things to be considered, viz.: when the conjunction occurs at V″ and E″, Venus being very near that part of her orbit where it crosses the plane of the earth's orbit. At this time, if an observer on the earth were to look at Venus, he would see her upon the sun's face. The same would happen if the conjunction took place on the side of the orbit opposite to V″ and E″. It is a matter of great importance to ascertain at what times this will occur; at what time the conjunction takes place when Venus is near that point (called the node) of her orbit. Now, whenever the conjunction occurs at V″ or on the side exactly opposite, then there is an opportunity

of seeing Venus on the sun's face. The most celebrated transits of Venus, as I have said, occurred in 1761 and 1769; and the next will occur in the years 1874 and 1882.

I will now proceed to show you the method by which these transits are made available for measuring the distance of the sun from the earth. I must first point out to you what we know and what we do not know. From observing the distance which Venus goes to the right and left of the sun, we do know the proportion of the distance of Venus from the sun to the distance of the earth from the sun. This must be remembered carefully. We do not know the absolute distance of the earth from the sun, nor the absolute distance of Venus from the sun, but we know their proportion. Now, with respect to the diameter of the sun: suppose that I have an instrument like a pair of compasses, and that I use this so as to be able to observe the apparent angular breadth of the sun. I apply the joint of the compasses to my eye; I direct one of the legs to the lower part of the sun, and the other to the upper part of the sun; I then know the apparent angular breadth of the sun; and from this I determine the proportion of the absolute diameter of the sun to the distance of the sun, and whatever distance I assume for the sun I must take the diameter proportionately to that distance. If the distance of the sun is one hundred millions of miles, the breadth of the sun (roughly speaking) must be one million; if the distance of the sun is fifty millions of miles, the diameter of the sun is half-a-million of miles. Let Figure 43 be a perspective view of the state of things in which the sun's distance is supposed to be one hundred millions of miles; and the breadth of the sun is one million of

miles. Let Figure 44 be a perspective view of the state of things in which the sun's distance is supposed

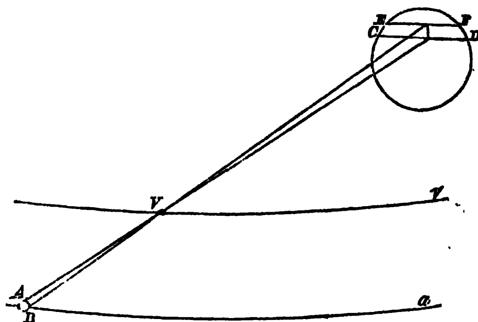


Fig. 43.

to be fifty millions of miles, and its breadth to be half-a-million of miles. In Figure 43, the distance of Venus from the sun must be seventy-two millions

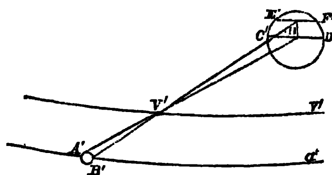


Fig. 44.

of miles; but in Figure 44, the distance of Venus from the sun is only thirty-six millions of miles. Of all these measures we do not know anything absolute, we only know their proportions.

But there is one measure which we do know accu-

rately. The diameter of the earth is nearly eight thousand miles, and from the knowledge of that we shall be able to determine all the others. It must then be carefully borne in mind that the diameter of the earth AB, in Figure 43, is the same as the diameter of the earth A′B′, in Figure 44.

Suppose, then, that there is to occur a transit of Venus: that is to say, a conjunction of the sun and Venus is about to take place, when Venus is near the node of her orbit, in which case Venus is seen to pass across the sun's face. We will suppose that the observation is to be made at two Observatories, one of which is near the North Pole of the earth, and the other near the South Pole of the earth. From the relation of the motions of different planets expressed in Kepler's third law (that the squares of the periodic times are proportional to the cubes of the distances), it follows that the periodic times increase in a greater proportion than the distances from the sun, and that the actual motions of the more distant planets in their orbits are slower. Thus, suppose one planet is 4 times as far from the sun as another; then its periodic time is 8 times as great (since the square of 8 is equal to the cube of 4); but the orbit which it describes is only 4 times as large; and thus it moves with only half the speed in that orbit. In like manner the motion of the earth in its orbit is slower than the motion of Venus in her orbit. Therefore, as the earth is moving in the direction from AB towards *a*, and Venus is moving with greater speed in the direction from V towards *v*, the inhabitants of the earth will see Venus moving apparently across the sun in the direction from E towards F, or from C towards D, (or in a retrograde direction). Now, first, let us examine in Figure 43 what will be the

apparent path of Venus over the sun's face, as seen at the point A, near the North Pole of the earth. Let a straight line be drawn from A through V till it meet the sun's face, and the end of that line will describe the path CD on the sun's face, (considering the sun's face as a flat disc, which will suffice for this purpose). Thus from the northern station we see Venus travelling along the line CD. In a similar way we find that, from the point B on the south side of the earth, Venus is seen to travel along the line EF.

Suppose the distance between the points A and B to be 7000 miles, and let us calculate the distance between the two lines CD, EF. The supposition is, that the distance of the earth from the sun is one hundred millions miles, and the distance of Venus from the sun seventy-two millions of miles, and consequently the distance of Venus from the earth twenty-eight millions of miles. It follows that the interval between the lines CD, EF (which must have the same proportion to AB that 72 bears to 28), is 18,000 miles.

We will now go on the supposition represented in Figure 44, that the distance of the earth from the sun is only fifty millions of miles, and therefore that the distance of Venus from the sun is thirty-six millions of miles, because, as I have said, the proportion of the distance of Venus from the sun to the distance of the earth from the sun is known beforehand, and the distance of Venus from the earth fourteen millions of miles; and let C′D′ be the path of Venus as viewed from A′, and E′F′ the path of Venus as viewed from B′; and, still supposing the distance between A′ and B′ to be 7000 miles, let us calculate what is the interval between C′D′ and E′F′. This interval is in the same proportion to A′B′ as 36 is to

14, and therefore it will be 18,000 miles, exactly the same as in Figure 43. Thus the interval between the two lines of the apparent path of Venus is the same, whether we suppose the earth's distance from the sun to be one hundred millions of miles, or fifty millions of miles.

But there is this cause of difference in their effects, that on one of these suppositions the whole diameter of the sun is one million of miles, but on the other it is only half-a-million of miles. Thus, as taken in absolute measures of miles, the parallel lines CD, EF, are just as far apart as the parallel lines C′D′, E′F′; but they are drawn across a circle whose diameter in one case is double what it is in the other. But, as viewed from the earth, the apparent diameter of the sun is the same on the two suppositions, but the lines CD, EF, (if we could see them painted on the sun), would *appear* nearer together, on the supposition of Figure 43, than on that of Figure 44. Now, inasmuch as in the two figures we make no difference of supposition as to the position of Venus as viewed from the earth's centre, the lines will cross the sun's disc in the same *general* position in both figures; and, therefore, they will meet the edge of the sun's disc at nearly the same angle*; and, therefore, as the interval between them is the same, the difference of length in miles between CD and EF is the same as the difference of length between C′D′ and E′F′.† But as the lines CD and EF are about double the length of C′D′ and E′F′, the proportion of the difference of lengths

* This will be true if the difference between the radii of the circles which are compared with one another be small compared with either radius.

† It is to be borne in mind that the interval between CD and EF is small in comparison with the diameter of the sun.

to the whole length in Figure 43 is only half what it is in Figure 44. Thus, suppose the position of the lines to be such that CD is longer than EF by one-thirtieth part: then C′D′ is longer than E′G′ by one-fifteenth part. And if the earth's distance from the sun is one hundred millions of miles, and if Venus be seen to cross the sun's disc in the direction which we have supposed, the difference of the times occupied by the passage of Venus over the sun, as viewed from A and from B, will be only one-thirtieth part of the whole time; but if the distance of the earth from the sun is only fifty millions of miles, the difference of times will be one-fifteenth part of the whole time.

Now we have come to something that can be compared immediately with observation. We can observe at the two stations A and B the whole time that is occupied in the passage of Venus across the sun's face. And having ascertained the whole time of transit at each of these stations, we can take the difference between the two times, and find what proportion it bears to the whole time. If, (supposing the lines to cross the sun's disc in the direction supposed above,) the difference is found to be one-thirtieth of the whole, then we conclude that the sun's distance is one hundred millions of miles; if it is one-fifteenth of the whole, we conclude that the distance is fifty millions of miles; and so from any other result of observation as to the difference of times occupied in the passage, we draw an inference as to the sun's distance.

It is now proper to remark that the observations cannot be made strictly as we have supposed, at two stations A and B, which preserve the same relative position during the whole transit, because the earth is during all this time revolving on its axis. And it will be worth while to remark how advantage may

be taken of this circumstance to increase the difference of times of passage of Venus over the sun's disc as seen at the two places, and thus to render the result more accurate. (For if, on the supposition that the sun's distance is one hundred millions of miles, we can by proper choice of stations increase the difference of times from 16 minutes to 24 minutes, and if the uncertainty in observation from chance errors is five seconds, then that uncertainty is $\frac{1}{192}$ of the whole in the former case, and $\frac{1}{288}$ of the whole in the latter case; and the proportionate uncertainty in the sun's distance will be the same.) The transit of Venus in 1769 occurred on June 3, a day very near to the summer solstice. The North Pole of the earth was turned partly towards the sun. Venus appeared to pass across the sun's disc from left to right, or in the retrograde direction. The earth was revolving from right to left, or in the way which we call direct. Now let Figure 45 represent the view which would be had by a person (for instance, an inhabitant of the planet Mars, if it had happened to be in the proper position) looking over the earth, and seeing the sun beyond it. To avoid confusion, Venus is omittted from this figure. The South Pole, P′, of the earth would be seen by him, but the North Pole, being nearer the sun, would be invisible. A station near the South Pole would, in the course of its diurnal revolution, describe a circle, of which the greater part (represented by the dark arc of a circle) would be towards his eye, and the smaller part would be on the

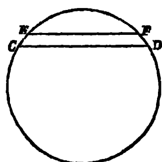




FIG. 45.

side next the sun. A station near the North Pole would describe a circle, of which the smaller part would be towards his eye, and the greater part towards the sun. The whole passage of Venus occupied about six hours. Now, it was possible to choose a station A (Wardhoe, in Lapland) such that, at the beginning of the transit Wardhoe was at A, that it then passed the dark part of its revolution, and arrived on the sunny side at *a* at the end of the transit; in other words, that the transit began in the evening and ended the next morning. And another station B (Otaheite) might be chosen, such that the transit began soon after Otaheite entered into the light at B, (or in the morning) and ended shortly before Otaheite came to the darkness at *b*. Now, we have already seen that Venus as seen from A described the longer path CD, merely because A is higher in the figure than B; but now we may see that, in consequence of A being to the left, Venus will be seen to enter at C sooner than if A had been in the central line; and, in consequence of *a* being to the right, Venus will be seen to leave the sun at D later than if *a* had been in the central line; and for both reasons, the time in which Venus appears to describe the path CD will be lengthened. In like manner, it will be found that, as seen from B and *b*, the time in which Venus appears to describe the path EF will be shortened. And, therefore, the difference of the whole durations of the transit as seen at the two stations will be considerably increased.

I have mentioned specially Wardhoe and Otaheite. Many other stations were used, but these were the best of all. The whole duration of the transit at Wardhoe was about 5 hours 54 minutes. The whole duration at Otaheite was about 5 hours 32 minutes.

The difference of durations was about 22 minutes. We may perhaps say that 7 minutes of this was caused by Wardhoe being north of the earth's centre, 5 minutes by Otaheite being south of the earth's centre, and 10 minutes by the circumstance that at Wardhoe the transit began near sunset, while at Otaheite it began near sunrise.

On computing, from the known dimensions of the earth, the distances of the points A,*a*,B,*b*, in miles, and finding (by the proportion which I have mentioned before) how distant in miles were the lines CD, EF, how much they differed in length, and how much they appeared to differ when respect was had to the different positions of the two stations at the beginning and end of the transit; and finding what must be the actual diameter of the sun in miles, in order that the difference between these lines CD, EF, thus altered for the changes of positions of the two stations, might bear to their whole length the same proportion which the difference of observed durations of transit bore to the whole duration of transit, and computing from this diameter of the sun in miles what his distance in miles must be to make his apparent angular diameter what we know it to be; it was found that the mean distance of the sun is 95,300,000 miles. If the distance of the sun had been only 47,650,000 miles, the difference of durations of transit at Wardhoe and Otaheite would have been 44 minutes.

The method of determining the sun's distance, which I have thus attempted to illustrate, is one of the most difficult subjects for a public lecture that I know; and if I have given you a few notions of it I shall be perfectly satisfied. I shall make some further remarks on this subject in the next lecture.

LECTURE V.

Recapitulation of Lecture IV.—Precession of the Equinoxes.—Lunar Nutation.—Aberration of Light.—Measure of the Distances of the Stars.

IN last evening's lecture I treated more especially of the measures of the distance of the moon and the sun from the earth; and first of the distance of the moon from the earth. I endeavoured to point out to you that the method of parallax, by which the distance of the moon from the earth is measured, might be illustrated by the combined operations of the two eyes in the head, by which the distance of objects near us may be approximately estimated; but that in measuring the distance of the moon from the earth, instead of making use of the two eyes in the head, we make use of two observatories on the earth, placed at a considerable distance from each other, from which places we observe the same celestial object. I then pointed out to you two places that are peculiarly adapted for this measure; one of these is the Observatory at the Cape of Good Hope, and the other is Greenwich or Cambridge, or any other European Observatory. At the European Observatory, by means of the mural circle, we observe the apparent angular distance of the moon from the North Pole, at the time when it passes the meridian; and we also observe on the same day, at the Observatory at the Cape of Good Hope, what is the apparent

angular distance of the moon from the South Pole of the heavens. The reason why we must make the observations in this way is, because at the European Observatory we can see the North Pole, while at the Cape of Good Hope they can only see the South Pole. The angle between the two celestial poles is necessarily 180 degrees exactly, because the directions of the lines drawn from the two places to the celestial poles are parallel to the same line, namely, the axis of the earth. I then remarked that the moon, as viewed from the Cape of Good Hope, does not pass the meridian precisely at the same time as when viewed at the Observatory at Greenwich or Cambridge. But the difference of time is well known; and we have only to take into account the change in the moon's place during the interval which elapses between the observations at the two places. That is done with very great accuracy, and we can reduce the angular distance of the moon from the South Pole, to what it would have been if it had been observed at the same time as at Greenwich. Thus we have, for the same instant of time, got the apparent angular distance of the moon from the North Pole, as seen at Greenwich or Cambridge, and the apparent angular distance of the moon from the South Pole, as seen from the Cape of Good Hope. The sum of these two angular distances, if a star were observed, would be 180 degrees; but when the moon is observed, the sum of these two angular distances is found to be more than 180 degrees, and the excess above 180 degrees is the effect of parallax. It is the same as the angle which is made at the moon by two lines, one drawn from the European Observatory and the other from the Cape of Good Hope. Here we have got everything in much the same state as when

measuring any distant object by means of a base line. For, from our knowledge of the form and dimensions of the earth (Figure 40), we know the length and position of the line GC; and the observations made at Greenwich and at the Cape of Good Hope give us the angles MGC and MCG; and thus we have the elements for computing the lines GM and CM, and then the distance EM can be found with little trouble.

I then added, that there is one cause of uncertainty, which is refraction, and which produces its effect in this way: we refer the observation of the moon at Greenwich to the North Pole of the heavens; and we refer the observation made at the Cape of Good Hope to the South Pole of the heavens. In deducing the real places of the moon from the apparent places, it is necessary to take into account the quantity of refraction which enters in these two cases. There is one calculation of refraction, amounting to a great many seconds, or, perhaps a minute or two, to be taken into account in the observations made at Greenwich; and another calculation of refraction, perhaps amounting to a like quantity, to be taken into account at the Cape of Good Hope. As I said before, refraction is the plague of astronomers, and owing to it, there is always a little uncertainty in the measurement of large angles on the celestial meridian. On that account it is desirable, if possible, to diminish that refraction. If we suppose that there is a star S, Figure 40, at a distance so great that its position is sensibly the same when seen from any part of the earth, and if two observers select the star by previous concert; and if the person at G observes with his instrument the place of the moon as well as that of the star, he finds how many degrees, minutes, and seconds,

the moon is below the star. This is the angle SGM, and the comparison by which it is determined is almost independent of refraction. By similar observations at C, the angle SCM is found with equal accuracy. And the difference between these angles gives the angle CMG with great accuracy. And it is this angle upon which the distance of the moon mainly depends.

I then explained that the calculation, in point of fact, is not made by treating MGC as one triangle in a survey, but by dividing the angle GMC into two parts by the line EM, and then assuming for trial a value of the distance EM and computing the angle EMG, and with the same assumption computing the angle EMC, and adding them together, and finding whether this sum agrees with the observed angle GMC; if it does not agree, the assumption of distance must be varied till it does agree. There is no difficulty in each of these computations; because, from the dimensions of the earth, it is easy to find the inclination of the line GE to the vertical H′GE′; and therefore from the observed angle H′GM the angle HGM is found; also the length GE is known; and the length EM is assumed for trial; and then the calculation of EMG is easy.

It is now proper to mention that astronomers very seldom refer to the actual length EM in yards or miles. I explained that the angle EMG is called the parallax of the moon at G, and the angle EMC is the parallax of the moon at C. Now, (referring for the present to the place G only,) if the line GM were perpendicular to the line EG, that is, if the moon were in the horizon as viewed from G, the parallax would be greater than in any other position of the moon, (supposing the distance EM not to be

altered.) This is called the *horizontal parallax.* And as the earth is not spherical, and therefore different places are at different distances from the earth's centre, the horizontal parallaxes will not be the same at different places; it is therefore convenient to fix on some one place as a standard. The place fixed on by the consent of astronomers is the equator; and the horizontal parallax of the moon at the equator is called the *horizontal equatoreal parallax.* This is the quantity used by astronomers in relation to the moon's distance; it is convenient for their calculations, and it amounts to the same thing as using the distance; for if the distance is known, the horizontal equatoreal parallax is known; or if the horizontal equatoreal parallax is known, the distance is known. The moon's horizontal equatoreal parallax varies (according to the moon's distance), from 54 minutes of a degree to 61¼ minutes; these correspond respectively to the distances 252,390 miles and 222,430 miles.

I then stated, that when every observation of the moon made at any one place is corrected for parallax, so as to inform us what would be the position of the moon as viewed from the centre of the earth, it is found that her orbit is sensibly a plane; and this conclusion may then be properly used as a basis for determining with greater accuracy the moon's horizontal parallax at that place. For the moon's angular distance from the Pole when she is nearest to it will fall short of 90 degrees, just as much as it will exceed 90 degrees when she is farthest from the Pole, provided that the proper correction for parallax is made to as to reduce the observed place to what it would have been as seen from the earth's centre. If the condition which I have mentioned is not satisfied, it

is a proof that the assumed value of horizontal parallax is wrong, and a new trial must be made. I mentioned to you that this method is interesting, not only as being in use at the present time, but also because it is the method first used by Greek Astronomers.

Both in the last and in the present century, the distance of the moon has been found with great accuracy by these methods. Its mean value may be roughly stated to be 240,000 miles, or about thirty times the breadth of the earth. It seems a long way, and it is a long distance to measure, considering that it is ascertained by the use of a yard measure. You will observe that it is really and truly measured so. A yard measure was used to measure the base in the trigonometrical survey; by means of this, and a series of triangles, a long line was measured on the earth; by knowing its length, and by making observations with the Zenith Sector, the form and dimensions of the earth were found; and by knowing the size of the earth, and by observing the angles which relate to parallax, the moon's distance is found. You see we have thus proceeded step by step from the yard measure to the moon's distance. We may thus picture to ourselves the distance of the moon. If a railway carriage travelled at the rate of 1000 miles a day, it would at that rate be eight months reaching the moon.

I then proceeded to point out to you by what means the measure of the sun's distance had been attempted; and first I pointed out to you some methods which have failed. In the first place, I remarked that the distance of the sun from the earth might apparently be measured, just in the same way as the distance of the moon from the earth, by observations at two distant observatories. This method

practically is inapplicable, because the uncertainty of refraction is great, for the air is in a heated state, and it is therefore in a state unfavourable for the observation; and because no star can be observed near the sun. In this case, a small uncertainty produces a far greater effect than in the case of the moon. Suppose, for instance, that there is an uncertainty in the observation which will produce an uncertainty of one second in the horizontal parallax. The smaller is the parallax, the greater is the distance in the same proportion. If, then, by this error, the moon's horizontal parallax is altered from 57 minutes to 57 minutes 1 second, the measure of distance is altered by only $\frac{1}{3421}$ part of the whole, or by 70 miles. But if the sun's horizontal parallax is altered from 9 seconds to 10 seconds, the measure of distance is altered by $\frac{1}{10}$ part of the whole, or by more than 9 millions of miles. On this account, the method of simple parallax entirely fails in ascertaining the distance of the sun.

I then mentioned another very ingenious method, one founded on the observation of the place of the moon when it is "dichotomized." This is a Greek expression used to denote that state of the moon when it is half illuminated. If we can fix upon that time exactly, we shall know that the angle at the moon made by lines drawn to the sun and the earth is a right angle, and if we can then measure the angle at the earth between the sun and the moon, and subtract that angle from 90 degrees, we shall have the angle at the sun made by lines drawn to the earth and moon; and from this we shall be able to compute the proportion between the sun's distance and the earth's distance. This method fails because the surface of the moon is so exceedingly rough.

I then pointed out a third method, which has been tolerably successful, (although it is not the one regarded as being the most accurate), namely, by observing the parallax of Mars, which moves round the sun in an orbit between those of the earth and Jupiter. It is founded upon these considerations. First, that from seeing how the planets go right or left of the sun, or how much their motion deviates from motion in a circle round the earth, the proportion of the distance of Mars from the sun to the distance of the earth from the sun is known quite independently of any knowledge of the absolute distances; and that in fact this proportion was known with considerable accuracy many centuries ago. In the year 1700 it was nearly as well known as it is now. Secondly, the proportion of the distances from the sun being known, it followed that the proportion of the distance of Mars from the earth to the distance of the earth from the sun was known. Suppose then, at a certain time, (I am obliged to say at a certain time because the orbit of Mars is very eccentric,) when the sun, the earth, and Mars, are in a straight line, suppose at that time we know the distance of Mars from the sun is four-thirds of the distance of the earth from the sun, it follows from that, that the distance of Mars from the earth at that time, is one-third of the distance of the sun from the earth. If we can by any method find the distance of Mars from the earth at that time, and if we multiply it by three, we shall get the distance of the sun from the earth. Thirdly, the distance of Mars from the earth can be obtained by the simple method of parallax, as in the first method for the moon; and with considerable accuracy. At two observatories, as in Europe and at the Cape of Good Hope, the position of Mars when on the

meridian, may be compared with some fixed star, the same star being observed at the European Observatory and at the Observatory at the Cape of Good Hope. I am afraid that it will seem that I am dealing in generalities in this matter; but all that I can endeavour to do is to make the principles of the computation intelligible. It is nearly impossible to go into details. The method which I have just described is not, however, the best method, although it has been used with tolerable success.

The fourth method is, by observing the transit of Venus over the sun's disc. The transit of Venus occurs rarely. In explaining this, it was necessary to point out that the orbits of the different planets are inclined to each other, as in Figure 42, where EE′ represents the orbit of the earth and VV′ that of Venus. You will observe that at V Venus is considerably elevated above the plane in which the earth moves. If the conjunction takes place at V, that is, if the sun, Venus, and the earth, are nearly in the line at S, V, E, still, however, they are not and cannot be exactly in the same direction; and if a spectator upon the earth looks at Venus, he will see her considerably above the sun. If the conjunction takes place at V′, she will be seen below the sun. But if the conjunction takes place at V″, Venus, the earth, and the sun, are exactly in a line when the conjunction takes place; at this time a spectator on the earth will see Venus on the sun's face as a black spot. I believe it is visible to the naked eye. With a telescope it is seen extremely well. I have seen Mercury, which is a much smaller body, on the sun; and a very beautiful black spot it is.

In the observation of which I am going to speak, it is necessary to know beforehand the time when

the conjunction will take place, that is to say, the time when Venus will be seen on the sun's face. In eight years Venus goes round the sun thirteen times with very considerable accuracy, but still not with perfect accuracy. Suppose, then, that a conjunction of Venus and the earth takes place, at a particular position of the two planets; eight years after that time there will be another conjunction, nearly but not precisely at the same place. In eight years after that there will be another conjunction at a point still more distant than the first, and thus the points of conjunction will recede gradually from V″, and it will be a long time before a conjunction occurs again, either at V″ or on the opposite side. Venus was seen on the sun's face in 1761 and 1769, at the position opposite to V″: she will next be seen on the sun's face in 1874 and 1882, at the position V″.'

I then proceeded to point out the principle of the method in which these conjunctions are used for determining the linear distance of the sun from the earth. Figures 43 and 44 represent the state of things at a transit of Venus on two assumptions: Figure 43 on the assumption that the distance of the earth from the sun is one hundred millions of miles, and Figure 44 on the assumption that the distance of the earth from the sun is fifty millions of miles. Venus moves in her orbit faster than the earth, and, in consequence, as the earth moves towards *a* or *a′*, and Venus moves faster towards *v* or *v′*, she will be seen when between the sun and the earth to move across the sun in the direction CD or C′D′. I remarked that, before the observation is made, one linear measure only is known, namely, the size of the earth; but that we do not know the distance of Venus from the sun, or the distance of the earth from

the sun, but we know that proportion to be as 72 to 100 nearly; and, similarly, that we do not know the absolute breadth of the sun, but we know that, whatever the distance of the sun may be, the breadth of the sun bears a certain proportion to that distance, namely, that it is nearly the hundredth part of the distance.

Then I pointed out that if the transit of Venus be observed at two points of the earth, A and B, or A′ and B′, which are 7000 miles apart, Venus will appear to describe on the sun's face the line CD or C′D′ as seen from A or A′, and EF or E′F′ as seen from B or B′, and that the distance between the lines EF and CD, or between E′F′ and C′D′, will be the same quantity in linear measure, namely, 18,000 miles, whatever be the supposition as to the distance of the sun; and therefore, as the *general* position of the lines on the sun's face must be the same on any supposition, and therefore they will cut the edge of the sun's disc at nearly the same angle, the difference of the lengths of the two paths CD and EF, or C′D′ and E′F′ must be the same number of miles; but, as the linear breadth of the sun is not the same on the two suppositions, and therefore the linear length of the lines CD or EF is about double the length of C′D′ or E′F′, it follows that the difference of their lengths bears a smaller proportion to their whole length in Figure 43 than in Figure 44; and therefore, the difference of the times occupied by the apparent passage of Venus, as seen from A and from B, bears a smaller proportion to the whole time on the assumption of Figure 43 than on that of Figure 44. It is plain that here we have something which will guide us immediately to a decision on the distance of the earth from the sun, if we can but make observations at two stations, as A and B. For, observing with telescopes and clocks the

entrance and departure of Venus on the sun's face at both places, and therefore ascertaining the whole duration of the passage at both places, and consequently the difference of durations; if that difference bears the same proportion to the whole time as what we have computed on the assumption of Figure 43, then the assumption of distance in Figure 43 is a true one; if the proportion of the difference to the whole time is the same as that computed in Figure 44, then the assumption of distance in Figure 44 is true; if neither of these agree, another assumption may be made which will come near the truth.

I then pointed out that the difference of durations (on any supposition of distance) may be much increased by choosing two stations, one as at A (Figure 45), such that it has nearly turned to the shady side of the earth when the transit is commencing, (or in other words, such that the transit begins shortly before sunset,) and has just turned to the illuminated side when the transit is ending, (or in other words, such that the transit ends shortly after sunrise,) as the former circumstance makes the beginning of the transit earlier, and the latter makes the end of it later, and therefore, the time occupied by the apparent passage along CD, which is the longer, is still more increased; and by choosing another station B, such that the transit begins in the morning and ends in the afternoon, in which case the time occupied by the passage along EE, and which is already the shorter, is still more diminished. And thus, upon any supposition of the sun's distance, the difference of the durations of the transit is increased, and therefore, in comparing the observed difference of durations with the computed difference, a small error of observed durations will be a smaller proportional part of the

whole, and therefore, the result for the sun's distance will be more accurate. I also mentioned that, in the transit of 1769, Wardhoe and other places in Lapland answered very well to the former of these conditions, and Otaheite and other places in the Pacific answered well to the latter; and that these in fact were the places of observation upon which the measure of the sun's distance principally depends.

I may now mention that, although the principles of the method are stated with most perfect correctness in the explanation given above, and although the process must be thus contemplated by an astronomer, in order to enable him to select stations in the most advantageous positions, yet an astronomer's calculation is not made in that form. His calculation is made entirely by the method of parallax. The process, strictly speaking, is algebraical; but it may be correctly described in the following manner. He assumes a certain value in seconds for the sun's horizontal equatoreal parallax; then from the known proportion of the distances of Venus and the sun, he computes the horizontal equatoreal parallax of Venus (the parallax being greater as the distance is less). Thus, if the sun's horizontal equatoreal parallax be assumed at ten seconds, and if it be known that at that time the distances of Venus and the sun from the earth are in the proportion of 28 to 100, then he must take the horizontal equatoreal parallax of Venus at thirty-five seconds and five-sevenths. Then, from knowing the earth's form, he computes the horizontal parallax of each at the place of observation; and then from knowing the apparent elevation of the sun and Venus, he calculates the actual parallax of each at the time of observation, that is, how much each of them is apparently depressed by parallax. Venus being

nearer than the sun is apparently more depressed than the sun, and is therefore moved by parallax off the sun's limb if she is lower, or upon the sun's limb if she is higher. From this he calculates, by a troublesome mathematical process, how much the time of Venus' entering or leaving the limb has been either accelerated or retarted by the effect of parallax. And this is done for both observations at every station. And thus he calculates what will be the difference of durations of the transit at different stations. All this is upon the supposition that the sun's horizontal equatoreal parallax has a certain value, say ten seconds. Then he compares the computed difference of durations with the observed difference of durations. If they agree, the sun's horizontal equatoreal parallax is ten seconds. If not, the true value will be found by this proportion. As the computed difference of durations : is to the observed difference of durations : : so is the assumed horizontal equatoreal parallax (ten seconds) : to the true horizontal equatoreal parallax. In this manner it was found that the sun's horizontal equatoreal parallax, when the earth is at its mean distance is eight seconds and six-tenths very nearly; and this corresponds to the distance which I have stated before, of about 95,300,000 miles.

It was supposed by Dr. Halley, when he gave the outline of this method of observation, that the difference of durations could be determined accurately within $\frac{1}{500}$ part of the whole difference, and that, consequently, the sun's horizontal equatoreal parallax could be found within $\frac{1}{500}$ part of the whole, and therefore that the sun's distance could be ascertained to within $\frac{1}{500}$ part. This is a most remarkable degree of certainty, considering how great the distance is, and that it is measured (so to speak) by one step

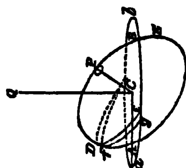
only from the dimensions of the earth, which, if the measure were conducted in any ordinary way, would be too small a base for so long a measure.

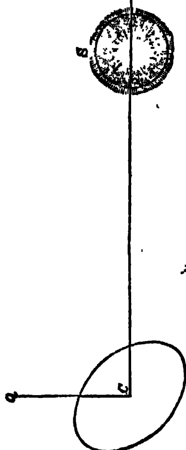
I said, that the distance of the moon from the earth is 240,000 miles, and that if a railway carriage were to travel at the rate of 1000 miles a day, it would be eight months in reaching the moon. But that is nothing compared with the length of time it would occupy a locomotive to reach the sun from the earth; if travelling at the rate of 1000 miles a day, it would require 260 years to reach it.

I will now proceed with explanations of some of the higher branches of Astronomy, which, though difficult, will be found most valuable: first, as instances of very important applications of the principle of gravitation; secondly, as showing the nature of some of the corrections to observations, which it is necessary to understand, in order to see clearly the different steps that must be made before we can arrive at a measure of the distance of the fixed stars.

I shall speak first of the Precession of the Equinoxes. This is a thing which was known as a fact of observation to the ancients. The person who discovered it was the Greek Astronomer Hipparchus, a hundred and fifty years before the Christian era. It was first explained by Sir Isaac Newton, by whom the principles of gravitation were made known. I will endeavour to convey the explanation to you; but it is a thing not to be done without much difficulty. For this purpose I must, in the first place, recal to your minds the laws of gravitation. The fundamental law of gravitation is this: that every particle of every body attracts every particle of every other body: one body does not attract another body as a mass, but every body attracts every other body as a collection

of separate particles: attracting every particle, independently of the others. It is essential to bear this in mind. The next thing is, that it attracts every particle with a force which depends upon the distance of the attracted particle from that body which is the cause of attraction; and the nearer that body is, the more strongly the particle is pulled by the attracting body. You will see then, that the sun attracts those parts of the earth next to it with a greater force than those parts near the centre of the earth. At A, Figure 46, the sun's attraction is stronger than at C, and therefore the sun is always acting upon the part A nearest to it, as if it were pulling it away from the earth's centre. This is not merely because the whole force which the sun exerts upon A is directed towards S, because if the sun pulled the centre and the surface of the earth equally, it would not tend to separate them; but it is because it pulls the part at A more than it pulls the central part C, and thus it tends to pull it away from the centre of the earth


FIG. 46.


FIG. 47.

towards S. In like manner, if the sun were to pull the centre C of the earth and the part B with equal force, it would not tend either to push B towards the centre or to draw it away from the centre; but, as it pulls the centre more powerfully than it pulls B, it does tend to separate them, not by pulling the opposite side B from the centre, but by pulling the centre from the opposite side B. The general effect of the sun's attraction, therefore, as tending to affect the different parts of the earth, is this: that it tends to pull the nearest parts towards the sun, and to push the most distant parts from the sun.

If the earth were a perfect sphere, this would be a matter of no consequence—it would produce tides of the sea, but it would not affect the motion of the solid parts. But the earth is not a sphere; it is flattened like a turnip, or has the form of which I have spoken to you under the description of a spheroid. Moreover, the axis of the earth is not perpendicular to the ecliptic; the earth's equator is inclined to the line joining the earth's centre with the sun at all times, excepting at the equinoxes.

Let us now consider the position of the earth at the winter solstice, represented in Figure 46. The North Pole is distant from the sun, the South Pole is turned towards the sun. This spheroidal earth, at this time, has its protuberance, not turned exactly towards the sun, but elevated above it. As I said before, the attraction of the sun is pulling the part D of the earth more strongly than it pulls the centre. What is the tendency of that action? The immediate tendency of that action is to bring the part D towards *a*, supposing *a* to be in the horizontal circle passing through S. In like manner, in consequence of the sun attracting the centre of the earth more than it

attracts the protuberance E, which amounts to the same thing as pushing the protuberance E away from the sun, there is a tendency to bring E towards *b*. It is very important that you should see this clearly: that if I were standing in the place S where the sun is, and if I had a line fastened by a hook to the place D, and if I pulled it, I should tend to bring that part towards *a*. The immediate tendency of this pull, therefore is, so to change the position of the earth that its axis will become more nearly perpendicular to the plane of the ecliptic. You might suppose then, that the effect of that pulling will be to change the inclination of the earth's axis to the line which connects the earth and sun. No such thing; the effect is entirely modified by the rotation of the earth. Undoubtedly, if the earth were not revolving, and if the earth were of a spheroidal shape, the attraction of the sun would tend to pull it into such a position that the axis of the earth would become perpendicular to the line SC; or (if in the position of the winter solstice) it would become perpendicular to the plane of the ecliptic; but, in consequence of the rotation of the earth, the attraction produces a perfectly different effect. Let us consider the motion of a mountain in the earth's protuberance, which, passing through the point *c* on the distant side of the earth, would, in the semi-revolution of half a day, describe the arc *c* D *e*, if the sun did not act on it, (*c* and *e* being the points at which this circle *c* D *e* intersects the plane of the ecliptic, or the plane of the circle *a b* that passes through the sun). While the protuberant mountain is describing the path *c* D *e* it is constantly nearer to the sun than the earth's centre is; the difference of the sun's actions therefore tends to pull that mountain towards S, and therefore, (as it cannot be separated

from the earth,) to pull it downwards, giving to the earth such a tilting movement as I have already spoken of; it will therefore, through the mountain's whole course, from *c*, make it describe a lower curve than it would otherwise have described, and will make it describe the curve *c f g* instead of the curve *c* D *e*. What will the result of that be? As it mounts from *c* to *f*, the sun's downward pull draws it towards the ecliptic, and consequently, renders its path less steep than it would otherwise be. At *f* it will be a very little lower than it would otherwise have been at D; but as the sun's downward pull still acts upon it till it comes to *g*, the steepness of its path between *f* and *g* is increased more than belongs naturally to its elevation at *f*, and becomes in fact the same as it was at *c*, or very nearly so; so that the inclination of the path to the plane of the circle *a b* is the same as at first. But, instead of crossing the circle *a b* at *e*, it will cross at *g*: in other words, it will, in consequence of the sun's action, come to the crossing place earlier than it would have come had the sun not acted. Now, consider what will be the motion of this protuberant mountain in the remaining half of its rotation, from *g* towards *c* again. In this part of its rotation, it is further from the sun than the earth's centre is; therefore the sun's action does in fact tend to push it away (as I have already explained); and as it cannot be separated from the earth, this force tends to push the mountain upwards towards the circle *b*, tilting the earth in the same direction as before; the mountain therefore, in this part, will move in a path higher than it would have moved in if not suffering the sun's action, and therefore it will come to its intersection with the circle *a b* sooner than it would if not subject to the sun's action. The inclination of

its path (just as in the former half of its rotation) will not be altered. Thus the effect produced by this action of the sun, in both halves of the rotation of this mountain, is, that it comes to the place of intersection with the plane of the circle *a b*, or with the plane of the ecliptic, sooner than it otherwise would. And whatever number of points or mountains in the protuberant part of the earth we consider, we shall find the same effect for every one; and therefore, the effect of the sun's action upon the entire protuberance will be the same; that is, its inclination to the circle *a b* or the plane of the ecliptic will not be altered, but the places in which it crosses that plane will be perpetually altering, in such a direction as to meet the direction of rotation of the earth.

I have spoken of this as if the protuberance were the only part to be considered. But in reality, this protuberance is attached to the remaining spherical part of the earth; and the action of the sun on the different masses of that spherical part balances exactly; so that, as regards it, we need not consider the sun's action at all. The effect therefore of that spherical part will be, to impede the motion which the protuberant part would otherwise have; not to destroy it, but to diminish it.

On the whole, therefore, the effect of the sun's action on the spheroidal earth will be, that the points at which the earth's equator intersects the plane of the ecliptic move very slowly in the direction opposite to that in which the earth revolves; but the inclination is not altered.

All that I have said here applies to the position of the earth at the winter solstice. But if we consider the earth at the summer solstice, as in Figure 47, we shall find that the effect of the sun's action is exactly

the same. The sun's greater attraction upon any part of the protuberance when nearest to the sun, at which time that part is, in the case of Figure 47, below the ecliptic, tends to raise it towards the plane of the ecliptic, and therefore it cuts the ecliptic sooner than otherwise it would. And the sun's smaller attraction upon it when furthest from the sun, producing the effect of a pushing force upon it when above the ecliptic, tends to make it descend to cut the plane of the ecliptic sooner than it otherwise would, and therefore, in both halves of the diurnal rotation (as at the winter solstice) the place of intersection of the earth's equator with the ecliptic will move in the direction opposite to the earth's rotation.

At the equinoxes, the plane of the earth's equator passes through the sun, and then the sun's action does not tend to tilt the earth at all, and consequently does not tend to alter the position of its equator at all; but at all other times the sun's action produces a motion, greater or less, of the intersection of the earth's equator with the plane of the ecliptic, in a direction opposite to the direction of the earth's rotation. And this is the motion called the Precession of the Equinoxes.

It is to be observed, that the principal part of precession is not produced by the sun, but by the moon. The moon's mass is not a twenty-millionth part of the sun's; but she is four hundred times as near as the sun. Still she does not pull the earth, as a mass, with more than a hundred-and-twentieth part of the sun's force. But, because the difference of the distances of the different parts of the earth from the moon bears a greater proportion to the whole distance than for the sun, the differential effect of the moon in pulling the near parts of the earth from the earth's

centre, and in pushing the distant parts of the earth from the earth's centre, is about treble the effect of the sun. And as the precession depends entirely on this differential effect, the precession produced by the moon is about treble that produced by the sun.

It will be well for you here to consider the consequences of this precession on the position of the earth's axis. In Figure 48, after a certain number

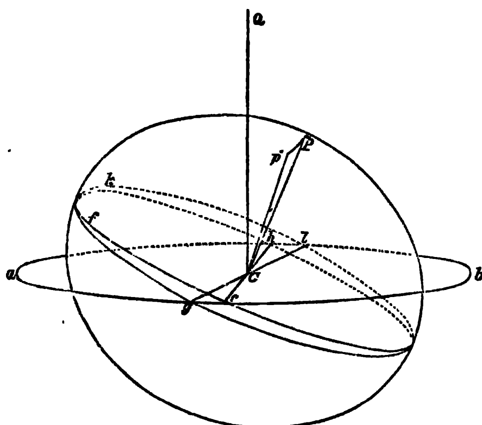


Fig. 48.

of days or years, the position of the earth's equator has changed from *e f h* to *g k l*; its inclination to the circle *a b* in the plane of the ecliptic remaining the same as before. The earth's axis of revolution must be always perpendicular to the plane of the earth's equator. From this it will be seen that the earth's axis has changed its position from such a direction as CP to such a direction as C*p*. If we draw a line CQ

perpendicular to the plane of the ecliptic, the inclination of the earth's axis to CQ will be the same as the inclination of the earth's equator to the ecliptic; and that, as we have already seen, undergoes no alteration. Consequently, the inclination of CP or C*p* to CQ is always the same. Therefore, we may represent the motion of the earth's axis by saying that it turns slowly round an axis perpendicular to the ecliptic, but keeping the same general inclination to it, in the direction in which the hands of a watch turn, (as viewed on the outside of a celestial globe,) or in what astronomers call a retrograde direction. Now, the Pole of the heavens is the point in the heavens to which the earth's axis is directed, and therefore that Pole is not absolutely invariable, but turns slowly in a circle in a retrograde direction (or in the same direction as the hands of a watch, as viewed from the outside of a celestial globe) round another point to which the line CQ is directed. The latter point is called the Pole of the ecliptic.

This motion of the earth's axis admits of a most remarkable illustration in the motion of a spinning top; and the more remarkable because the forces which act in that case are of an opposite character to the forces which act on the earth, and the effect which they produce is of an opposite character to the effect produced on the earth. The earth's axis being inclined to the line CQ, in Figures 46, 47, and 48, we have seen that the *immediate* tendency of the sun's force upon the earth is in all cases to bring the earth's axis CP *nearer* to CQ, and that if the earth had no motion of rotation, this force would bring the earth's axis CP *nearer* to CQ; but that, in consequence of the earth having a motion of rotation, the effect really produced is, that the earth's axis CP revolves slowly *round* CQ

in the direction *opposite* to the direction of rotation. Now, in Figure 49, let CP be the axis of a spinning

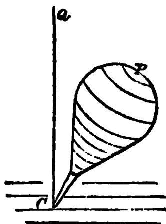


FIG. 49.

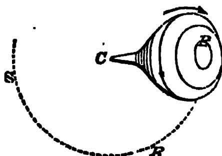


FIG. 50.

top, CQ the vertical line; the immediate tendency of the force of gravity is to bring the axis CP *further* from CQ (or to make the top fall); and if the top were not spinning, it would make CP recede *further* from CQ; but it will be found that, in consequence of its spinning, the inclination of CP to CQ does not sensibly alter (till the spinning motion is retarded by friction), but CP revolves slowly *round* CQ in the *same* direction as the direction of rotation. So that if Figure 50 represent a view of the top from above, if the top be *spun* in the direction marked by the arrow, its axis will *reel* in the direction PRS. This, as I have said, is strictly analagous to the precessional motion of the earth, although, from the *immediate* tendency of the forces being of an opposite kind, the *ultimate* effect is of an opposite kind.

The following astronomical effects of this precession deserve attention.

First, the celestial equator, which, as I said in a former lecture, is the great circle in the heavens which at every point is 90 degrees from the Pole, is in fact

in the same plane as the equator of the earth, whose axis (perpendicular to its equator, or making an angle of 90 degrees with every part of the equator) is directed to the Pole. Therefore, as the earth's equator changes its intersections with the plane of the ecliptic, the celestial equator also changes its plane and changes its intersections with the ecliptic. These intersections, as I mentioned, are called the first point of Aries and the first point of Libra: and one of the co-ordinates by which the place of a star or other body is defined, technically called the Right Ascension, is the interval of time between the passage of the first point of Aries over the meridian, and the passage of the star or other object over the meridian. But as this first point of Aries travels to the right, it passes the meridian every successive year earlier (with respect to the stars) than it would have done if it had been stationary; and therefore, the right ascensions of stars (for the most part) increase a little every successive year.

Secondly, as the place of the Celestial Pole changes from year to year, the North Polar distances of the stars change from year to year; some of them increase and some diminish.

The annual amount of precession, although a formidable quantity in delicate astronomical observations, is a very small quantity for ordinary observers. The annual motion of the first point of Aries is about fifty seconds in a year: it will require about 26,000 years to perform the entire revolution. The change in the distance of a star from the North Pole does not in any case amount to twenty-one seconds in a year. But these are quantities so large that we must be perfectly acquainted with their laws and magnitudes when we treat of small changes in the places of stars not exceeding one or two seconds.

The next thing which I have to mention, as one of the calculations which must be applied to observations in order to obtain an accurate result from them, is the nutation of the earth's axis. The nutation of the earth's axis may be described as arising in this way. I have explained that the action of the sun will produce a motion in the earth's equator and in the position of the earth's axis, principally at and near to the times of the winter solstice and the summer solstice, when either the North Pole or the South Pole of the earth is turned towards the sun. It matters not whether the North or South Pole is turned towards the sun, for the tendency is to produce equal motion of the axis, and in the same direction, in both cases. But at the time of the equinoxes, when neither Pole is turned towards the sun, the sun's attraction has no tendency to produce precession. From this you may easily see that the precession of the equinoxes goes on more rapidly in summer and in winter than in the intermediate months; there is therefore a certain irregularity in the precession, and this irregularity is one part of the solar nutation. The other part arises from the circumstance that the inclination of the earth's axis to the ecliptic, though unaltered *on the whole*, yet suffers slight changes from one instant to another; as clearly appears from the foregoing explanation.

I stated also that the moon produces a considerable part of the precession. There is a very small irregularity of precession produced by the moon in different parts of her monthly revolution, similar to that produced by the sun in different parts of his apparent yearly revolution. But the principal irregularity arises in this manner. The moon does not move in the plane of the ecliptic, but in an orbit inclined to the plane of the ecliptic; and this orbit (from an effect

of the sun's attraction, exactly similar to the motion of the earth's equator produced by the sun's attraction) moves so as to change the place of its intersection with the ecliptic, performing a complete revolution in 19 years. Therefore, during nearly half of this time, the moon's orbit is inclined to the ecliptic, in the same way as the earth's equator, (but not so much,) as at *m n*, Figure 51 ; and therefore the moon's path is little inclined to the earth's equator. And during nearly the other half of the time, the moon's orbit is inclined to the ecliptic in the way opposite to the earth's equator, as at *p q*, Figure 51, and the moon's path is much inclined to the earth's equator. In the former state, the moon's force to tilt the earth is small, and the precession goes on slowly ; in the latter, the moon's force to tilt the earth is great, and the precession goes on rapidly. The consequence of this is, that there is a very sensible irregularity in the motion of the earth's axis every 19 years, and the place of the Pole is irregularly shifted in different ways through more than 18 seconds. This is lunar nutation. As the right ascensions and North Polar distances of all stars and other objects are affected by this irregular disturbance of the Pole, it is necessary to take it into account in comparing observations made at one time with observations made at another time.

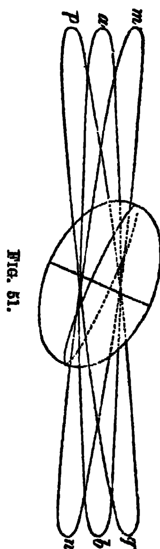


Fig. 51.

These corrections, then, (precession and nutation,)

are two of the irregularities of which it is necessary to take account in obtaining accurate results from observations; but there is a third; which is greater in its magnitude than nutation, and totally different in its nature. It is the aberration of light. It was long ago made out that vision is produced by something coming from the object to the eye, and that this something comes from the object to the eye with a definite velocity. Now, in consequence of this light coming from the object to the eye with a definite velocity; and in consequence of the earth's moving with a definite velocity; by the combination of these two things, there is produced a disturbance in the visible place of every object not connected with the earth which we look at. Perhaps one of the simplest ways of giving an idea of the effect of this combination, in relation to the aberration of light, will be to refer you to the chance experiment which suggested the theory of aberration to one of my predecessors (Dr. Bradley), by whom in fact the aberration of light was discovered and reduced to law. He says, he was being rowed on the Thames, in a boat which had a small mast with a vane at the top. At one time the boat was stationary, and he observed, by the position of the vane, the direction in which the wind was blowing. The men commenced pulling with their oars, and he observed that, at the very time they commenced pulling, the vane changed its position. He asked the watermen what made the vane change its position? The men said they had often observed the same thing before, but did not pretend to explain the cause. Dr. Bradley reflected upon it, and was led by it to the theory of aberration of light. I may here offer a slight illustration of it, which every person may observe if he walks out in a rainy day. If you can choose a day when

the drops are large, then if you stand still for a moment, and observe the direction in which the drops are falling, when there is little or no wind, you will see that the drops fall vertically downwards; but if you walk forward, you will see the drops fall as if they were meeting you; and if you walk backward, you will immediately observe the drops of rain falling as if they were coming from behind you. This is an accurate illustration of the principle of the aberration of light. I will now offer another. In Figure 52, let A be a gun in a battery, from which a shot is fired at a ship DE that is passing. Let ABC be the course of the shot. The shot enters the ship's side at B, and passes out at the other side at C. But in the meantime the ship has moved from the position DE to the position *d e*, and the part B where the shot entered has been carried to *b*. Now, if the ship were a sentient and reflecting being, when it perceived that the path which the shot made through it, entering at *b* and going out at C, was in the inclined direction *b* C, it would say, "The shot came from somewhere ahead." You will see in this, the effect of the combination of the movements; that the shot appears to have come from a part further ahead than it would have seemed to come from if the ship had been at rest. And you will also see that the inclination of the apparent direction of the shot *b* C to the true direction BC depends on the proportion of *b* B to BC, that is, on the proportion of the velocity of the ship to the velocity of the shot. The greater

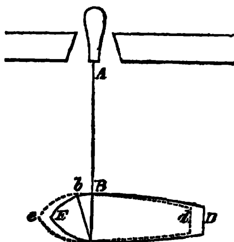


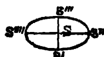
FIG. 52.

is the velocity of the shot, the smaller will be the space b B described by the ship while the shot is passing across her, and therefore the smaller will be the angle b CB between the apparent direction of the shot and its real direction.

The same thing happens with regard to the effect of the motion of the earth on the apparent path of light, and it will produce an apparent change in the places of the stars. And if we find that there is such an apparent change, it will be a certain proof that the earth is in motion; but if we find the change to be small, it will prove that the velocity of light is much greater than that of the earth.

Now I will point out to you the visible effect of the aberration of light upon the place of a star. The immediate interpretation of the consideration which I have mentioned is this. In whatever direction the earth is moving, the apparent position of any star which we are looking at, is displaced in the direction towards which the earth is moving. In Figure 53, let C be the sun, E′,E″,E‴,E⁗, the earth in four successive positions of its orbit (viewed in perspective), its motion at each place being in the direction of the arrow drawn there; S the true place of a star. Then, in consequence of the aberration, when the earth is at E′, as its motion is in the direction of the arrow drawn from E′, the light coming from the star will enter the eye of a spectator or the tube of a telescope, not as if it came from S, but as if it came from s', the line Ss' being parallel to the arrow at E′; and therefore the observer, when the earth is at E′, does not see the star at S but at s'. In like manner, when the earth is at E″ he sees the star at s''; when at E‴ he sees the star at s'''; and when at E⁗ he sees the star at s''''. Thus you will see that, in

every position of the earth, the star's place is affected by the aberration of light; and from this cause every



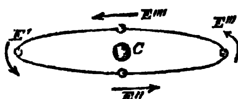


FIG. 53.

star apparently describes a small circle every year parallel to the earth's orbit. It is a minute circle; its angular diameter, as seen without any foreshortening, is found to be about forty seconds. This quantity is, however, so serious that it cannot be omitted in the computation of any observation whatever.

From the measure of the apparent semi-diameter of the small circle described by the star, corresponding to the angle BC*b* in Figure 52, we are able to compute the proportion of the earth's velocity to the velocity of light; and we find that the velocity of light is about 10,000 times as great as the earth's velocity in its orbit, or about 200,000 miles in a second. In other words, light travels a distance equal to eight times the circumference of the earth between two beats of a

clock. This is a prodigious velocity, but the measure of it is very certain.

These three quantities, (precession, nutation, and aberration,) are the corrections to a star's apparent place, which it is necessary for us to take into account in every observation of a star, at whatever part of the earth it is observed; and besides these, it is necessary at every place to apply the proper correction for refraction, which may be different at every different place of observation. Having obtained these elements of calculation, we can proceed at once with the measure of the distance of the fixed stars.

In Figure 54, let E′,E″,E‴,E⁗, be four positions of

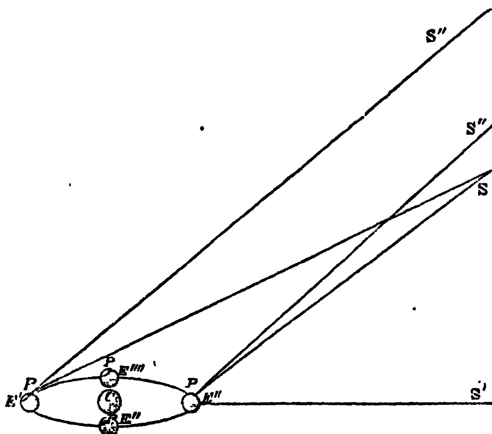


FIG. 54.

the earth in its orbit (seen in perspective), P a place of observation, S a star, (the earth being in such a

part of its rotation that the meridian of P passes through the star); also let S′ be another star in the plane of the earth's orbit, and in the direction corresponding nearly to the earth's solstitial position. And suppose (in conformity with the assertion that we have made all along, but which we shall now subject to the severest proof) that the earth's axis remains strictly parallel to itself in its motion round the sun, with no other motion than those which we have described as produced by precession and nutation. And suppose that with a mural circle at P we observe the zenith-distances of the stars S and S′ when they pass the meridian of P, and apply the proper corrections for refraction; and then, by applying corrections for the effects of aberration, we find the place in which the star would have been seen, if unaffected by the earth's velocity: and by applying corrections for precession and nutation, we find the zenith-distances which the stars would have had if the position of the earth's axis had not been affected by precession and nutation. Now, if our assumption (that the earth's axis has no motion but those depending on precession and nutation) be correct, the result of the observation of the star S′, whatever be its distance, will be, that its corrected zenith-distance when observed on the meridian will be the same whether the earth be at E′, E″, E‴, or E⁗. This is found to be strictly in agreement with the results deduced from actual observation, so that it is certain that the earth's axis has no motion but those depending on precession and nutation. Moreover, for the vast majority of stars in all parts of the heavens, when the same corrections are applied, the corrected meridional zenith-distances are found to be the same whatever be the position of the earth in its orbit; and this proves,

both that the earth's axis has no motion except those of precession and nutation, and that the stars are at an inconceivable distance.

But there may be other stars, as S, whose distance we have some reason for conjecturing to be not so enormously great. Now, the only way in which we can measure its distance is one strictly analogous to that used for measuring the distance of the moon; with this difference, that we cannot observe from two places at once. On account of the immense distance of the stars, it would be necessary to observe the place of the star from two positions, as far distant as the breadth of the earth's orbit; but we cannot do that. We can, however, observe the position of the star from the earth when the earth is in two positions, as E′ and E‴, on opposite sides of the earth's orbit; that is, at times half a year apart.

I have used, as an elucidation of parallax, the effect of the two eyes in the head. If you have your head in any fixed position, and you shut one eye, you cannot determine accurately the distance of an object; but if you open both eyes, the distance is seen immediately. But with one eye, a person can judge of distance very well, if he moves his head. In like manner, one observer on the earth can observe the distance of a star, provided he takes advantage of the change of places at different times; that is, provided he allows his eye to be moved round for him by the revolution of the earth round the sun; it is, however, necessary for us to be fully possessed of every element for correction of the star's place, so as to clear it of every source of change, except the difference of apparent place depending on the star's distance and the earth's place in its orbit. This is the reason why I have deferred the mention of this measure until I had

mentioned the subjects of precession, nutation, and aberration. The stars' places most sensibly change from circumstances unconnected with the parallax. We must know these accurately beforehand; and knowing these accurately beforehand, we proceed as follows.

We observe the star with the mural circle while the earth is in the position E′. We apply the corrections for precession, nutation, aberration, and refraction; and we shall know what the corrected position of that star should be half a year hence, as observed when the earth is at E‴. Now, suppose we go to this second state of things, and when the earth is at E‴, we observe the meridional zenith-distance of the star, we correct it for refraction, precession, nutation, and aberration. Now, do these two corrected zenith-distances agree? Are the stars (after all these corrections are applied) seen exactly in the same direction when the earth is at E′ and when it is at E‴? All calculations for these accidental causes of disturbance being effected, the result is this. For the vast majority of stars we do not discover any sensible difference; the difference is, at any rate, exceedingly small; the stars are so far off that, for the vast majority of them, we can see no difference in the directions of the line E′S and the line E″S. There are some stars, however, that are not at so great a distance, so that the inclination of these lines to each other can be ascertained; but the angle is exceedingly small, and is measured with much difficulty. In the southern hemisphere, there is the bright star of the Centaur, (Alpha Centauri,) for which it would seem that the inclination of the two lines from the opposite sides of the earth's orbit to the star, is an angle of two seconds and no more.

An angle of two seconds is that in which a circle $\frac{6}{10}$ of an inch in diameter would be seen at the distance of a mile. This is the star which shows the greatest parallax of all. The parallax of the bright star of Lyra is not more than a quarter of a second. Struve, at the Observatory of St. Petersburgh, has deduced, as he thinks, from observations, that for stars of the second magnitude the general average of parallax is $\frac{1}{10}$ of a second. This is so small an angle that it is almost impossible to answer for it. Supposing, however, that it is $\frac{1}{10}$ of a second, then the distance of the star from the sun is two million times as great as the distance of the earth from the sun. It seems almost inconceivable that we should be able to measure, even in a rough way, a distance so great.

I will only mention one more thing. There is one correction upon which I said there was a little doubt, and that is that troublesome thing, refraction. It is one of those things which throws a doubt upon every observation of a delicate kind. Refraction enters here, because we must necessarily observe the zenith-distance of the star; and in comparing observations of zenith-distance at opposite times of the year, there is this unfortunate circumstance; the same star which is observed on the meridian in the day-time at winter will be seen on the meridian at night in the summer; or the star which is observed in the night in the winter will be observed in the day-time in the summer, when the state of the air is very different; so that the amount of refraction at the two observations will be very different, and we cannot determine the correction to the zenith-distance accurately, so as to answer for $\frac{1}{10}$ of a second between the observations. Under these circumstances, this determination of a difference between the observations at E′ and E‴,

amounting to only $\frac{3}{10}$ of a second, is more than I can undertake to answer for. In consequence of that uncertainty, another method has been introduced, admitting of far greater accuracy; it is by comparing two stars whose declinations are nearly the same. And here we fall upon another method, very similar to that which is used for measuring the distance of the moon. Suppose we have two stars S and S″, and suppose that the star S″ is at such an immeasurable distance that we cannot see in it any change of position. But suppose that I think it possible that the star S has a sensible parallax, these two stars being seen in nearly the same direction. I have already mentioned that we have obtained the parallax of the moon with the greatest accuracy, by comparing it with a fixed star, which is seen nearly in the same direction. We get rid of the uncertainty of refraction in this case, as the moon and the star are seen near to each other, and are therefore affected with almost exactly the same refraction. In like manner, if we compare a near star with a distant star, seen nearly in the same direction, we get rid of the uncertainty of refraction; and we also get rid of precession, nutation, and aberration; because they produce sensibly the same effect on both stars. Now, if I suppose S to be near, and S″ to be at such an enormous distance that it will have no sensible parallax; then when the earth revolves round the sun, I have only at E′ to observe the angle S″E′S between the two stars, and then in another position E‴ to observe the angle S″E‴S between the two stars; and, because E′S″ is sensibly parallel to E‴S″, the difference between these two measured angles is the angle E′SE‴.

This is the method which the celebrated Bessel, of Königsberg, used for determining the distance of the

small star, known by the name of No. 61 in the constellation Cygnus. He found that the change in the place of that star, as viewed from E′ or from E‴, produced by parallax, is about $\frac{6}{10}$ of a second; and this corresponds to a distance of 660,000 times the radius of the earth's orbit, or 63,000,000,000,000 miles. Enormous as this distance is, I state it as my deliberate opinion, founded upon a careful examination of the whole of the process of observation and calculation, that it is ascertained with what may be called in such a problem considerable accuracy.

The distance of the stars of the second magnitude, founded upon Struve's conclusions to which I have already alluded, is not far from two millions of times as great as the distance of the sun from the earth. In this determination I have much less confidence. The distance of Alpha Centauri, if reliance may be placed on the observation, is only two hundred thousand times as great as the distance of the sun from the earth.

LECTURE VI.

Recapitulation of Lecture V.—Velocity of Light deduced by Römer from observations of the Eclipses of Jupiter's Satellites.—Proper motion of Stars.—Motion of Solar System in space.—Theory of Gravitation.—Methods of computing Attraction.—Perturbations of the Moon.—Mutual Perturbations of the Planets.—Long Inequality of Jupiter and Saturn.—Calculation of Figure of the Earth from Pendulum Experiments.—Experiments on the Density of the Earth.—Schehallien Experiment.—Cavendish Experiment.—Weight of the Earth.—Weight and Density of the Sun.—Weight of some Planets and of the Moon.—Conclusion.

IN the lecture of yesterday evening, the first subject to which I alluded was the Precession of the Equinoxes, in reference to its mechanical causes. This is a thing so important, partly in itself and partly in connection with the causes which produce it, that I have no hesitation in speaking of it again. The thing which I particularly intended to convey to you was this: that if we consider the attraction of the sun upon the earth, and if we consider that the earth is not a sphere, but has a flattened turnip-like shape which we call an oblate spheroid; if we also remark the laws which apply to gravitation, namely, that the force which the sun exerts is greater the nearer the body is to it, and that the law of gravitation is to be understood as applying to every particle, not to the body as a mass; that it attracts the earth not as a whole, but as a number of parts separately; if we mark these things, we find that the sun attracts that

swelling part of the earth which is nearest to it more powerfully than it attracts the central part of the earth; and that it attracts the centre of the earth more powerfully than the parts further off. The sun, therefore, is endeavouring to pull, as it were, the nearest part of the earth from the centre towards the sun; and it is endeavouring to pull the centre from the distant parts towards the sun, which is the same as saying that it is pushing this distant part from the sun. Now, if the earth were a complete sphere, the pulling at any part above the ecliptic would not disturb its motion; because there would be a corresponding pull on the corresponding part below the ecliptic; but, inasmuch as the earth is not a complete sphere, but has this flattened, turnip-like shape, protuberant at the middle; at the time of the solstice this protuberant part in the direction of the sun being above the ecliptic, then the extraordinary pull which the sun makes on that place is not balanced by a corresponding pull at the part below the ecliptic, because there is no protuberant part there on which the sun can act; and the action of the sun on that part which is above the plane of the ecliptic tends to pull it down towards that plane.

I remarked that the first effect of this would seem to be, to change the inclination of the axis on which the earth revolves, and to bring that axis more nearly perpendicular to the ecliptic. And that would be the effect, if the earth were not revolving on its axis; but in consequence of the revolution of the earth round its axis, a totally different effect is produced. I illustrated that by calling your attention to the motion of a single point, as for instance, a mountain at the earth's equator. While that mountain is near the sun and above the ecliptic, the force of which I have

spoken tends to pull it downwards. And as the rotation goes on, the forces still tend to pull it downwards and downwards, until at last it comes to meet the plane of the ecliptic at a point sooner than it otherwise would. This takes place with regard to the attraction of the sun on every part of this protuberance. The tendency of this force is to bring every part which happens to be above the ecliptic lower and lower towards the ecliptic; and to make its path intersect the circle *acb*, Figure 48, sooner than it otherwise would. It amounts to this, that at the earth's equator the motion of each point is affected by such forces, that it tends constantly to come to its intersection with the plane of the ecliptic sooner than it otherwise would; or, to speak in other words, that intersection travels backwards to meet the rotatory motion of the earth. The same thing (as I fully explained) will happen if we consider the action of the sun on the distant parts of the earth, which I represented as being equivalent to a pushing force.

I then mentioned to you that the moon produces a larger part of precession than the sun does, although the moon is so very much smaller than the sun, (only $\frac{1}{20,000,000}$ part of the sun). She is, however, 400 times nearer than the sun; and this makes her whole attraction, in proportion to her mass, 160,000 times as great as the sun's; still her whole attraction is only $\frac{1}{125}$ of that of the sun. But the important thing to be remarked in the explanation above given is, that precession is not produced by the whole attraction of the sun or moon upon the earth, but by the difference between the attractions which they exert upon the earth's centre and upon the earth's nearest surface. For the moon, the proportion of the distance of these parts is nearly as 60 to 59, and then the difference of

the attractions is about $\frac{1}{30}$ of the whole attraction of the moon. But, for the sun, the proportion of distances is nearly as 24,000 to 23,999, and then the difference of the attractions is about $\frac{1}{12,000}$ of the whole attraction of the sun. The consequence is, that that difference of attractions of the moon is actually three times as great as that of the sun, and the precession produced by the moon is three times as great as that produced by the sun.

This is the mechanical explanation of the precession of the equinoxes. It was discovered as a fact by Hipparchus, a Greek Astronomer, one hundred and fifty years before the Christian era; it has been recognized ever since by all astronomers, and is now known with very great accuracy; and, in all probability, when Sir Isaac Newton first applied the theory of gravitation to the explanation of the movements of the solar system, the explanation of this discovery of Hipparchus' was one that struck his mind, and that of his contemporaries, more than any other.

The next subject which I pointed out as an important one in connection with observations, was nutation, and I described it in this way: that nutation is a want of uniformity in precession. For the explanation of precession, I had taken the position of the earth at one of the solstices. At this time the earth's equator is much inclined to the line connecting the sun with the earth's centre, and the precession of the equinoxes is going on very rapidly. But if the earth were in the equinoxial position, then the sun would shine equally on the North and South Poles, and the protuberance of the earth would be directed exactly to the sun; and the action of the sun upon that protuberance would not tend to change the position of the globe. From these causes it will be seen that

precession is not uniform. But in our calculations for the application of a correction to the places of the stars, as dependent on precession, it is convenient to begin in the first place by using a precession increasing uniformly with the time. And therefore, inasmuch as precession does not increase uniformly with the time, we are obliged to apply a correction to the precession computed as uniformly increasing, in order to take into account the inequality (both in the place of intersection of the equator with the ecliptic, and in the inclination of the equator to the ecliptic) with which precession goes on at different times; and that correction is the quantity called solar nutation.

There is, however, a much more important want of uniformity called lunar nutation, which I described in this way. The precession produced by the moon depends on the inclination of the moon's orbit to the earth's equator; and this inclination is not uniform. For the moon revolves in an orbit inclined to the ecliptic; and the sun attracts the earth and the moon, and disturbs the motion of the moon with regard to the earth when he acts unequally on the two, nearly in the same way as the sun disturbs the motion of the supposed mountain at the earth's equator; and the effect produced is similar, namely, that the intersection of the moon's orbit with the ecliptic travels backwards; and thus, at periods nearly ten years apart, it is alternately more inclined and less inclined to the earth's equator. And thus, for nearly ten years the precession is going on too fast, and for an equal period it is going on too slowly; and thus a considerable inequality is produced in the motion of the intersection of the equator with the ecliptic. Moreover, for nearly ten years the moon's orbit is so inclined that the moon's action tends to diminish the inclination

of the earth's equator to the ecliptic, and for an equal time it tends to increase the inclination of the earth's equator to the ecliptic; and thus a considerable inequality is produced in the inclination of the earth's equator to the ecliptic.

The general effects of precession will be more easily conceived if, instead of considering the intersection of the equator with the ecliptic, we consider the motion of the earth's axis; as the change in the intersection of the equator with the ecliptic must produce a change in the position of the earth's axis as directed towards the stars. In consequence of this, the real Celestial Pole does change among the stars. The bright star of the Little Bear, for instance, now our Polar Star, is not at the same distance from the Pole now at which it was one hundred years ago. In the time of Dr. Bradley, the Polar Star was more than two degrees from the Pole; now it is one and a half. In the course of a great many centuries the Celestial Pole describes a circle among the stars, and different stars successively take the position of the Polar Star. About 4500 years ago the Polar Star was the bright star of the constellation Draco.

The third subject which I mentioned was the aberration of light; an effect produced by the combination of the earth's motion with the motion of light. I endeavoured to illustrate this in several ways. One was this: if in a summer shower you stand still and watch the rain, you will see it falling in its proper direction; but if you walk forward, you will see the drops falling in an inclined direction, as if they were meeting you; and if you step backwards, you will see immediately that the drops of rain appear to be falling as if they were coming from behind you. As another illustration, I supposed that a ship is sailing past a

battery, and that a shot is fired at the ship, and I remarked that the direction which the shot takes through the ship is not a direction exactly corresponding to that in which it is fired, but has an inclined direction, which inclination depends upon this, that after the shot has entered the first side of the ship, and before it comes out at the second side, the ship has advanced sensibly. The magnitude of the inclination depends therefore on the proportion of the velocity of the ship to the velocity of the shot. From this it is plain that if we know the extent to which the apparent direction of that line of the motion of the shot was changed when passing through the ship, we shall have the means of computing the proportion of the velocity of the ship to the velocity of the shot. Now this is a case strictly analogous to the motion of light. The earth is travelling along, and whilst it is so travelling along, light comes upon it from different objects, for instance from the stars. And the effect is the same as in the case of the ship; that in consequence of this motion of the earth, the light appears to come, not from the real place of the star, but from an ideal place of the star, which is in advance, as estimated by the direction of the earth's motion. If we know in what direction the earth is moving, the light of the star appears to come from a point more in that direction than it should.

I then endeavoured to point out to you the influence which this would have on the apparent places of the stars. We have an earth revolving in an orbit round the sun. The place of the star then will not appear always the same, but will always be found in a circle, whose centre is the true place of the star, the line from the true place to the apparent place being always in the direction in which the earth is moving. If

we can observe the star in different seasons of the year, we can infer from our observations how much the place of the star is perverted by this effect of aberration; we shall see how much the apparent path of the light is inclined to the true path of the light, as in the analogous instance of the breach made through the ship. Thus we have the means of comparing the velocity of the ship with the velocity of the shot, or the velocity of the earth with the velocity of light. And the result of the observation is this: that the place of the star is disturbed one way or the other in different directions at different seasons of the year, twenty seconds and one-third. The inference from this is, that the velocity of light is ten thousand times as great as the velocity of the earth in its orbit. The velocity of light is perhaps the most inconceivable of all things; the velocity is so enormous, 200,000 miles in a second.

But these are things which we must often look at with suspicion. What I have stated seems at first an indirect way of getting at these results. Even by a person properly conversant with these matters, such results are hardly received without additional confirmation. There are phenomena which give confirmation, which I will now explain. Jupiter has four satellites. Their orbits are, in proportion to his diameter, comparatively small. Our moon is at such a distance from the earth that she is not eclipsed very often; her distance being about thirty times the breadth of the earth. Jupiter's satellites are comparatively close to him; so close that three out of the four are eclipsed every time they go round. On watching the appearances of Jupiter, one of the most remarkable things observed is, the eclipses of the satellites, (first seen by Galileo). When the earth is

in one position with respect to Jupiter, we see the satellites go into the shadow; that is, we see them disappear without any apparent cause. In another position we see them come out of the shadow; that is, we see them begin to appear in the dark space at a short distance from Jupiter. Figure 55 is adapted

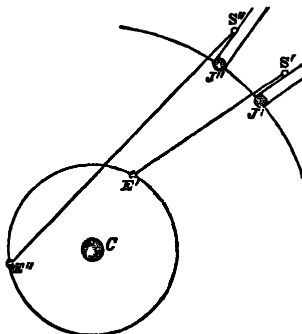


FIG. 55.

to the supposition that the satellites are coming out of the shadow. Suppose that C is the sun; E′, E″, the earth in two positions; J′, J″, Jupiter, in two corresponding positions. The time which is most favourable for the observation of Jupiter's satellites is that when the earth is nearly between the sun and Jupiter, as at E′, because then Jupiter is seen nearly the whole night. In a short time after the invention of telescopes, Galileo and other astronomers observed the satellites, and found that their eclipses could be observed with great accuracy, and registered them with great care. They were able in no long time to form tables and calculations of the eclipses of Jupiter's satellites. These occurred principally when the earth

and Jupiter were in such a position as E′J′. The earth went travelling on in its orbit, and came to such a position as E″. Jupiter, who is very slow in his motions, travelled perhaps as far as J″ in his orbit. And now came the remarkable thing: it was found that, when the earth came to such a position as E″, the tables and preliminary calculations upon which had been founded the predictions of the eclipses of the satellites would not apply. The eclipses of the satellites invariably occurred later than they ought to have done. This occurred year after year, and it was a long time before people could guess at the cause. Every time the earth came to that part of its orbit in which it is nearest to Jupiter, the eclipses of the satellites happened as predicted: every time the earth approached the part of its orbit furthest from Jupiter, the eclipses of the satellites occurred later than predicted. At last a very celebrated man, a Dane, of the name of Römer, gave the explanation, that in these latter observations the earth was further off from Jupiter than at the time when those observations were made on which the tables and calculations were founded: and therefore, the light from Jupiter had to travel over a path longer by very nearly the breadth of the earth's orbit. Upon this calculations were made, and the result was this: that the time occupied by the passage of light across the semi-diameter of the earth's orbit is 8*m.* 18*s.*; and therefore the time occupied by the passage of light across the whole breadth of the earth's orbit is 16*m.* 36*s.* Upon applying corrections, proportionably to the distance, to the observations made in other positions, it was found that they all harmonized perfectly well, and no doubt was left of the truth of the result, that the time the light occupies in travelling from the sun to the earth is 8*m.* 18*s.*

The question for us now is this: does this determination of the velocity of light agree with the deduction made from the aberrations of the stars? We found that the light travels ten thousand times faster than the earth moves in its orbit; if the light occupy 8*m.* 18*s.* in coming from the sun to the earth, does that imply a speed ten thousand times as great as the speed of the earth? The fact is, that the two calculations, though perfectly independent, support each other with the greatest nicety; and there is no doubt of the correctness of the measure of the velocity of light.

The subject on which I then proceeded at the lecture yesterday was the measure of the distances of some of the fixed stars; and I observed in the first place, that it was necessary for me to premise these various things, namely, the explanation of precession, nutation, and aberration, and for this reason: that the apparent places of the stars are disturbed by them to a very sensible degree, both in right ascension and in North Polar distance; and that the very utmost accuracy is necessary in everything relating to the observations upon which the measure of the distances of the stars are to be founded. I assume that we now know the meaning of the term "parallax." Between the apparent places of the moon, as seen at one point of the earth and as seen at another point of the earth, there may be a difference of a degree and half, or more. Now when we have a degree and half of difference, an error of a second is of no particular consequence. The parallax of the sun, as found in the way described in a former lecture, is a very much smaller quantity, between 8 and 9 seconds; that is, there is a difference of 8 or 9 seconds in the sun's places, as seen at the centre of the earth and on the

surface. A second or two either way becomes of great importance. But when we come to treat of the distances of the stars, we find that the parallax which can be exhibited, even in the difference of the position of the stars as seen when the earth is at different parts of its orbit, (which is frequently called the *annual* parallax,) is not certainly in any case two seconds; and in every case but one is certainly less than a single second. An error of a fraction of a second is here of very great importance; it deranges the whole of the results. It is therefore of the utmost importance to take into account the quantity of precession, which amounts to 50 seconds in the year; nutation, which amounts to 9 seconds one way or another; and aberration, which amounts to 20 seconds in one direction or another. All this we must know perfectly well before we enter into the question as to the parallax of the stars.

Now, with regard to the observations of the distances of the stars, I remarked that those observations were, in character, not exactly similar to the observations which are made for ascertaining the distances of the sun and the moon and Mars, which we have spoken of before. In all other cases we were able to plant Observatories at different parts of the earth, and from these different parts of the earth to make observations at the same instant on the subject in question, whether it was the distance of the sun, or of the moon, or of Mars, that was to be measured. But we cannot do so with respect to the stars, and for this reason: the mere observation of the stars from two points of the earth does not present any sensible difference whatever. We have no reason to believe that the apparent places of a star, as seen from one part or another of the earth, are different by the

ten-thousandth part of a second. We can, however, do this: we can make observations on the place of a star, when the earth is in two positions widely different. If S is a star, Figure 54, whose distance we wish to estimate, we can do it if we can observe it when the earth is in the position E′, and also in the position E‴. There is half a year between these observations; but still, if we can take into account all the changes in the star's apparent place that happen in the course of half a year, we shall be able to get some notion of the real change in the position of that star which arises solely from the different position of the earth in its orbit. Now let us see what we have to do. The position of the star at each of these times is affected by the three causes I have mentioned: precession, nutation, and aberration. The way in which we must reduce the observation of the star is this: we must make such a correction to the place in which we do really see the star, as will reduce it to the place in which we should have seen the star at a certain time, provided that the variable parts of the corrections were done away with. For instance, I observe the star on the 1st of February; precession has been going on for many centuries; I do not, however, reduce the place of the star very far back, I only apply the correction which is due to the change between January 1 and February 1, and thus refer the star's place to the beginning of the year. I then remark that nutation has sensibly disturbed the star's place. I do not, however, apply the correction (as regards nutation) to show where the star would have been on the 1st of January; but I apply a correction to show where the star would have been seen if there were no such thing as nutation at all. I take

the same steps as regards aberration; that is to say, I correct the place of the star so as to show where it would have been seen if there were no such thing as aberration. I have then got my observation of the star corrected for these various causes in such a manner, that its place is totally freed from the disturbances of a periodical nature, and from the change of precession which it has undergone from the 1st January. About the 1st August I repeat the observation. I then apply, to this observation, corrections of the same kind; that is to say, I correct it for the change which, by precession, the place of the star has undergone since the 1st January. I apply the correction for nutation on August 1, so as to show what its place would have been if no nutation existed; and I then apply the correction for aberration on August 1, so as to show what its place would have been if no aberration existed; and then from the observation made on August 1, I have the position of the star as it would have been seen at this part of the earth's orbit; all the periodical causes of disturbance being removed, and the precession existing in just the same state as it did on the 1st January. There then remains only one disturbing cause, and it is that which depends on the position of the earth in the different parts of its orbit. In order to find whether the place of the star is really affected by that cause, the step most convenient is to observe the North Polar distance of the star. If the magnitude of the earth's orbit be not anything sensible, as viewed from the star, then the North Polar distance of the star, corrected as I have mentioned, will be the same in these two positions of the earth. But if the North Polar distance of the star, duly corrected, be not the

same at these times, then it is to be inferred that there is a sensible difference in the direction of the lines E′S and E‴S.

Now you will observe, that there is a good deal assumed in this. In the first place, when we observe the North Polar distance of the star, the thing to which we refer it is the position of the Pole in the heavens. The Pole of the heavens is in reality that point of the heavens defined by continuing the earth's axis in a straight line to the region of the stars, so that when we observe the North Polar distance of the star, we do really determine the position of that star in relation to the position of the earth's axis. We assume, therefore, that we can account fully and accurately for the change of position which the earth's axis has undergone, and which arises from precession and nutation. Now, there is always a very minute uncertainty about these, which it is desirable to get rid of. Another cause of uncertainty arises from aberration; on this account there is always an uncertainty of a fraction of a second, which enters into the observation. There is also in most cases another cause of uncertainty; it is that of which I have spoken so frequently, refraction, which is such a trouble to astronomers. Nearly every observation which we make upon the positions of the stars is affected by refraction, and after making all proper allowance, we cannot always answer for the results. These considerations serve to cast some doubts on the observations made for determining the distances of the stars. Still there is one star of which the parallax seems to be determined with considerable accuracy, and that is the bright star of the Centaur, Alpha Centauri. It appears certain that this star has an annual parallax of one second (using the term "annual

parallax" to denote the extreme difference of apparent positions of a star, as seen from the sun on the one hand, and from the earth on the other hand), which amounts to this: the distance of the star is 200,000 times greater than the distance of the sun from the earth. That is a thing, however, which requires many observations for its verification.

I then mentioned another way in which the distances of the stars may be ascertained; a method which is free from all those defects of which I have spoken. This method is by the observation of two stars, of which one is believed to be very much nearer to the sun than the other. For then we may assume that the distant star will have no sensible change of place from parallax, depending on the position of the earth in its orbit. And then, in observing the stars from the various parts of the earth's orbit, we can compare the apparent place of that star which we believe to be the nearer with the place of the other. Practically this is of importance. The refraction, precession, nutation, and aberration, are sensibly the same; and there is no uncertainty whatever from the computation of the various quantities which cast so much uncertainty on the results derived from other observations. This is the method pursued by Bessel in determining the distance of the star 61 Cygni. He measured the angular distance of this star from two small stars near it, by means of an instrument, called the Heliometer, well known on the Continent, but of which there was at that time no specimen in England. With this he determined the parallax of the star 61 Cygni to be one-third of a second; that amounts to the same as saying that the distance is 600,000 times greater than the distance of the earth from the sun. It is deserving of

attention that 61 Cygni is a double star; but we know from long observation that the two stars partake of the same motions, and probably are a connected system like the earth and moon, and therefore we speak of them and of their distance as if they were only one star.

I have here spoken repeatedly of our supposition that some stars are nearer to us than others. The grounds of this supposition are generally the amount of what is called the *proper motion* of the stars. Upon comparing the places of the stars, as we observe them in different years, and applying the corrections for precession, nutation, and aberration, so as to reduce every observation of every star to what it ought to exhibit on the first day in the year, agreeably to the common practice of astronomers, we find that a great number of the stars have what is called *proper motion.* We are obliged to give up the idea of fixity entirely. The term "fixed stars" is a good term for young astronomers to use; but the vast majority of the stars which have been well observed, seem to have a motion of their own, and that is known by the term proper motion. In all good catalogues of stars there is reserved a column, distinguishing the proper motions of the stars, showing the direction in which the stars appear to be moving through other stars, and the amount of their motion in a year. This has only been discovered after many years' observation; it is in every case a small quantity; but still in most instances the quantity has been correctly ascertained. Those of Sirius and Arcturus are pretty large; but the largest known are those of two small stars, 61 Cygni (whose motion is nearly three seconds in a year), and a star known by the name of Groombridge, 1830, (whose motion is nearly four

seconds in a year.) The attention of astronomers has therefore been directed to both those stars, and it appears certain that the former has sensible parallax, and probable that the latter has parallax of a somewhat smaller amount.

In closing this account of the method of measuring the distance of the stars, I will only remind you that I have redeemed my pledge of showing how the distance of the stars is measured by means of a yard measure, and I will very briefly recapitulate the principal steps. By means of a yard measure, a base-line in a survey was measured; from this, by the triangulations and computations of a survey, an arc of meridian on the earth was measured; from this, with proper observations with the Zenith Sector, the surveys being also repeated on different parts of the earth, the earth's form and dimensions were ascertained; from these, and a previous independent knowledge of the proportions of the distances of the earth and other planets from the sun, with observations of the transit of Venus, the sun's distance is determined; and from this, with observations leading to the parallax of the stars, the distance of the stars is determined. And every step in the process can be distinctly referred to its basis, that is, the yard measure.

Before dispatching the subject of observation of stars, I will make one remark. The proper motions of many are very irregular in direction and magnitude; but with regard to some others there is a rude regularity which may be conceived in this way. I speak of it in connection with what is supposed to be the motion of the solar system in space. Suppose that I am walking through a crowd of people, or through a forest, if I keep my attention on those objects that

are exactly in front they do not appear to change their places; but if I look at the objects to the right or to the left, they appear to be spreading away to the right or to the left. Even if I did not know that I was moving myself, yet by seeing these objects spreading away, I should infer with tolerable certainty that I was moving in a certain direction. Now if it should appear that, taking the stars generally, we can fix on any direction and see that the stars in that direction do not appear to be moving, but that the stars right and left appear to be moving away from that point, then there is good reason to infer that we are travelling towards that point. This speculation was first started by Sir William Herschel. He found a point in the heavens, in the constellation Hercules, possessing this property, that a great majority of the stars about this constellation had not any sensible proper motion, but that the stars right and left of it had apparently motion to the right and left respectively. He inferred from this that the solar system was travelling in a body to that point, and this notion has been generally received amongst astronomers. I believe that every astronomer, who has examined it carefully, has come to a conclusion very nearly the same as that come to by Sir William Herschel, that the whole solar system is moving bodily towards that point in the constellation Hercules. But it is a thing on which the computation is not very accurate, and it will probably remain inaccurate for many years to come. This is the last subject which I have to mention in regard to the fixed stars.

I shall now proceed to the last division of my lectures: a general view of the evidence that applies to the theory of gravitation, with which is inseparably

connected the determination of the masses of the different bodies of the solar system. And here I must observe that, on entering minutely into this subject, it is impossible to take one thing alone. When I take the theory of gravitation, I must begin by taking the evidence relating to the laws of motion, for these were described and defined long before the theory of gravitation was expounded. Now the laws of motion, in the shape in which they have been commonly expressed, are these: in the first place, if a body be started in motion, and if no force act upon it, that body will continue in motion in the same direction and with the same velocity. Of all things in the world, this is the most difficult to prove immediately. It is obvious that we cannot put a body in motion so that it shall go on in one unvaried direction, and that it shall go on for ever, for we cannot put it in motion in a place where no force will act on it; and we cannot observe it through infinite space and infinite time. This is one of those instances in which we can examine a law only in connection with other laws. We must investigate by profound mathematical process what will be the effect of combining this law with others, so that we may observe whether the results, which are produced practically, agree with the results which we have found from the mathematical process.

For instance, one of the cases which we can observe is, that where rotatory motion is continued for a very long time, and where the velocity very slowly diminishes, as the motion of a wheel or spinning top. Contrivances have been made for the purpose of spinning tops and wheels in the exhausted receivers of air pumps. They go on spinning and spinning, and the motion seems as if it would never

come to an end. Now, how does this apply to the first law of motion, that a body moves in the same straight line and with the same velocity? All I can tell you is this : if it is true that each part of a body would, if unconstrained, move steadily in a straight line, and if (by the connection of the parts) each part is constrained to move in a circle, then it appears by mathematical investigation that the body will revolve with a uniform velocity; but if it were not true, then the body would not move in a circle with a uniform velocity. There is another instance which is perhaps still more remarkable. I allude to the motion of a pendulum. The motion of a pendulum backwards and forwards is a result of the first law of motion, taken in combination with the disturbing force of gravity ; this motion of the pendulum being the most permanent of all that are the subjects of ordinary experiments. If a pendulum be properly constructed, mounted with a steel edge (like that of the best balances) moving on a flat plate of hard agate, and if it be set in the exhausted receiver of an air pump, it will go on for 24 or 30 hours, without the action of anything to keep up its motion. But still it seems a very strange thing to infer, from this backward and forward motion, the law which asserts that the pendulum would go on continually in the same direction in a straight line, if there were nothing to disturb or counteract the operation of that law. Upon making the proper mathematical investigation, it is found that the only way of explaining the motion of the pendulum is, by saying that it would go on in a straight line continually, if it were not acted on by certain causes which we are able to take into account.

It is right you should understand how the matter

stands. When we speak of the evidence of these things, we cannot give the evidence simply as it applies to any one law, but we can give it in combination with other laws.

The second law of motion we have endeavoured to illustrate (page 104) by apparatus, which showed that when a ball was allowed to fall freely it was carried to the ground in the same time as a ball projected horizontally. The third law of motion relates to the effect of pressure, with which I have no occasion however to trouble you at present.

Having said so much on these subjects, we now come to gravitation. It is necessary to make this mention of the laws of motion first, because the movements connected with gravitation are but an instance of the application of the laws of motion to the movements produced by a certain force. The planets and satellites are in motion, and, according to the first law of motion, they would move on in straight lines, if they were not bent aside by some force. This force, according to the theory of gravitation, is the attraction of another body. Let us now examine whether such a force, following the law of decreasing as the square of the distance increases, will account for some of these motions; and we will begin, as Newton did, with the moon.

The moon's motion with respect to the earth is influenced (according to this theory) almost entirely by the attraction of the earth; because though the sun attracts both the earth and the moon, yet it attracts them nearly in the same degree, and therefore produces little disturbance in their relative motions. And though the moon attracts the earth, still the moon is so much smaller than the earth, that we may omit the consideration of that at present. We shall

however hereafter allude to the effects of both these circumstances. Now the principle of calculation in this and all similar cases will be the following: in Figure 56, let MN be the arc which the moon describes in her course round the earth E, in one hour, or one minute, or one second, or any other short time that we may choose to fix on (we shall, for the present, take one second). If no force had acted on the moon, she would have moved in a straight line M*m*. Therefore the force with which the earth has attracted the moon, has drawn her from *m* to N, or through the space *m*N. We must therefore compute the length of *m*N; we shall then know how far the earth's attraction draws the moon in one second; we also know how far the earth's attraction makes anything at the surface of the earth fall in one second; and the proportion of these will give the proportion of the earth's attractions in these two different places. Now, considering the moon as moving in a circle whose semi-diameter is her mean distance, EM is 238,800 miles. Also the whole circumference of the moon's orbit is 1,500,450 miles; and her periodic time is 27 days, 7 hours, 43 minutes; hence the length of the line M*m*, which would have been described in one second if no force had acted, is 0·6356 of a mile. With these two lengths of the sides of the right-angled triangle EM*m*, by the usual rule of squaring the two sides, adding the squares together, and extracting the square root of the sum, the hypotenuse E*m* is found to be 238800·0000008459 miles. Therefore the line *m*N is 0·0000008459 of a mile, or 0·0536 of an inch; and this is the space

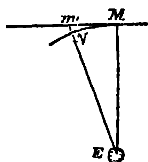


FIG. 56.

through which the earth's attraction draws the moon in one second.

Now at a place on the earth's surface, which is 3,959 miles from the earth's centre, it is found by experiment, that a stone falls 193 inches in one second. And it is found (by a difficult mathematical investigation), that if our theory is true in this respect, "that the attraction of the stone to the earth is produced by the attraction to every particle of the earth," (see page 175,) then the attraction of the whole earth (considered to be a sphere) will be the same as if the whole matter of the earth were collected at its centre. Thus the question upon which the explanation of the moon's motion by gravitation must depend is this: the earth's attraction at the distance of 3,959 miles draws a body 193 inches in one second, and the earth's attraction at the distance of 238,800 miles draws the moon 0·0536 inch in one second. Are these effects of the earth's attraction *inversely as the squares of the distances? They are,* almost exactly. To make them exactly so, the space through which the earth draws the moon should be 0·05305 inch. Now it is found (by a process which I cannot hope fully to explain to you) that the two circumstances which I mentioned, namely, the moon's action on the earth and the sun's disturbing force, do exactly explain this small difference; so that it is certain that the attraction of the earth which causes a stone to fall, and the attraction of the earth which bends the moon's path from a straight line to a circle, are really the same attraction, only diminished for the moon in the inverse proportion of the square of her distance.

I have used, for the time through which I compare the attracting power of the earth in the two cases

(upon the moon and upon the stone), one second, because the experiment of the distance through which a stone falls in one second, and its result are the most familiar to our minds. If I had used one minute, I should have found for the space through which the earth draws the moon 0·0030452 of a mile; or if I had used one hour, I should have found 10·963 miles; and I should have had corresponding numbers for the space through which a stone would fall in the same time. These numbers, it is to be observed, increase in the proportion of the squares of the times; and so do those for the fall of a stone (for a stone falls in two seconds four times as far as in one second; in three seconds it falls nine times as far as in one second; and so on).

I will now compare the spaces through which the sun's attraction draws the planets in one hour, and as an instance, I will take the earth and Jupiter. In Figure 57, *let EF be the path described by* the earth in one hour, E*e* the path in a straight line which the earth would have described in one hour if nothing had disturbed it. JK the path described by Jupiter in one hour. J*j* the path which Jupiter would have described in one hour if nothing had disturbed it. Then *e*F is the space through which the sun's attraction has drawn the earth in one hour, and *j*K is the space through which the sun's attraction has drawn Jupiter in one hour; and we shall proceed

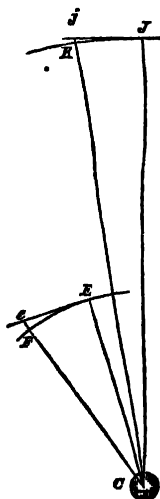


FIG. 51.

to find the proportion of *e*F to *j*K. Now taking CE as 95,000,000 miles, the circumference of the earth's orbit is 596,900,000 miles, which the earth describes in 365·26 days; and therefore the line E*e* which is the earth's motion in one hour, is 68,091 miles. Adding the square of CE to the square of E*e*, and extracting the square root of the sum, we find that CE is 95000024·402 miles; and therefore *e*F, the space through which the sun draws the earth in an hour, is 24·402 miles. For Jupiter, CJ, is 494,000,000 miles; the circumference of its orbit is therefore 3,104,000,000 miles; which is described in 4332·62 days; therefore J*j*, the motion in one hour, is 29,850 miles; and the length of C*j*, found in the same manner, is 494000000·9019 miles; and *j*K, the space through which the sun draws Jupiter in one hour, is 0·9019 miles. Hence, the attractive force of the sun on the earth is to the attractive force of the sun on Jupiter, in the proportion of 24·402 to 0·9019. But if we compute from the rule of the inverse square of the distances, what would be the proportion of the force of the sun on the earth to the force of the sun on Jupiter, we find that it is the proportion of 24·402 to 0·9024. These proportions may be regarded as exactly the same, the trifling difference between them arising mainly from the circumstance, that I have only used round numbers for the distances of the two planets from the sun. And thus for these two planets it is true that the strength of the sun's attraction is inversely proportional to the square of the distance of the attracted body from the sun.

If I had compared any two planets, I should have arrived at exactly the same agreement. And generally, I may state (though I cannot at present

demonstrate it to you), that whenever this rule (see page 126) is found to hold "the squares of the periodic times of several bodies moving round a central body are proportional to the cubes of the distances of the several bodies from that central body," then it will be found, by a process exactly similar to that which we have gone through, that the effects of the central body's attraction at the different distances are inversely as the squares of the distances. Now, this law (that the squares of the times are proportional to the cubes of the distances) was discovered by Kepler, long before the theory of gravitation was invented, to hold in regard to the times and distances of the planets in their revolutions round the sun. Moreover, in regard to the four satellites of Jupiter the same law holds. For we are able without difficulty to observe their periodic times; we are able also (by observing the transits and the difference of Polar distances of Jupiter and each satellite, or by other methods) to ascertain their apparent angular distance from Jupiter; and from this, knowing the distance of Jupiter from the earth in miles, we can compute the distance of each satellite from Jupiter in miles; and we find that the squares of their times are proportional to the cubes of their distances; and therefore the attraction of Jupiter upon his several satellites is inversely proportional to the squares of their distances from him. In like manner, it is found that the attraction of Saturn upon his seven satellites is inversely proportional to the squares of their distance from him; and, as far as we can examine, the same law holds with regard to the attraction of Uranus on his satellites. Thus, for every body which we know, around which other bodies revolve, the force of attraction of the central body on the different bodies that revolve

round it is inversely proportional to the squares of their distances.

But, though it is thus established that the attraction of the central body on the different bodies follows that law, is it true that its attraction on the same body alters in the inverse proportion of the square of the distances, when the distance of that body is altered? It is quite certain. But so difficult are the mathematical operations by which this is proved, that I can do little more than refer you (as I have done once before) to the results. The following, however, are the principal steps.

The planets do not revolve in circles but in ellipses, (see page 126,) and therefore, the distance of each planet from the sun undergoes considerable alteration. Kepler's second law of planetary motion was this: that if we draw a line from the sun to a planet, that line passes over equal areas in every successive hour; that area not being the same for different planets, but being constantly the same for the same planet; or, which is the same thing, it describes areas proportionable to the times. Now, if we assume the first and second laws of motion to be true, we find that this equal description of areas compels us to admit that the planet is attracted towards the sun; but it does not give us any information as to the law of the attractive force. But the circumstance that the planets move in ellipses, with the sun in one focus of each ellipse, settles this question. It has already been proved that the attractive force must be directed to the sun, that is, to the focus of each ellipse; and then it is proved by a mathematical investigation that if a planet moves in an ellipse, and if the force is directed to the focus of the ellipse, that force in different parts of the orbit must be inversely as the square of the

distance from the sun. Thus it is proved that the attraction of the sun on each planet, at its different distances, is inversely proportional to the square of the distance. The same thing also is proved with regard to those planets which have satellites; for several of the known orbits of satellites are elliptical (the others being circular).

There is, however, another very remarkable set of bodies, each of which in its motions sometimes goes nearer to the sun than any other known body, and sometimes passes further from the sun than any other known body; I mean the comets, the explanation of whose motions is one of the most remarkable of Newton's discoveries. A very few comets, (not more than five or six,) it is now known, move in very long ellipses, and return periodically to our sight; and to these the same remarks apply which have been above applied to the motion of planets. But at the time when Newton investigated the motions of comets, the idea of periodical comets was totally unknown, and Newton's investigations in regard to comets proceeded entirely on the supposition that the comet did not return. It is difficult for me to attempt to explain here how the orbit of a comet is investigated; the best way perhaps will be to give you something like a history of the thing.

When Newton had investigated the forces which apply to the motion in an ellipse, it was very natural that he should endeavour to see whether the same law of force (namely, that the force is inversely as the square of the distance) which accounts for motion in an ellipse, would account for motion in any other curve. You will see easily that there are two things upon which the motions of a planet depend. One is the force of the attraction of the sun; the other the

velocity with which the planet is set going. It is quite conceivable that if a planet were started with very great velocity, it might go away and never come back. The idea which Newton suggested was, that the motion of a comet was of that kind. And, upon pursuing the investigation, he found that a body subject to the attraction of a central body (as the sun) might, if the force varied inversely as the square of the distance, describe the curve called the parabola, (see page 131); but no other law of force would account for the description of such a curve. The form of the parabola is represented in Figure 58, C being the sun; and this curve it is evident, possesses two of the peculiarities which most markedly distinguish the motions of comets; it comes very near to the sun at one part, and it goes off to an indefinitely great distance at other parts.

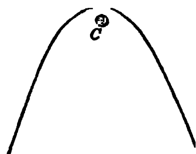


FIG. 58.

Now, when Newton had found out that the same laws of gravitation which were established from the consideration of elliptic motion would account for motion in a parabola, he began to try whether the parabola would not represent the motion of a comet. It was found, that by taking a parabola of certain dimensions, and in a certain position, the motions of the comet which had been observed most accurately could be represented with the utmost precision. Since that time, the same investigation has been repeated for hundreds of comets, and it has been found in every instance that the comet's movements could be exactly represented by supposing it to move in a parabola of proper dimensions and in the proper position, the sun being always situated at a certain point

called the focus of the parabola. This investigation tends most powerfully to confirm the law of gravitation; showing that the same moving object, which at one time is very near to the sun and at another time is inconceivably distant from it, is subject to an attraction of the sun varying inversely as the square of the distance.

But if it is true that every particle of matter attracts every other particle of matter, with a force varying inversely as the square of the distance, the effects of these attractions will be shown in other ways besides influencing the periodic revolutions of planets round the sun, or of satellites round their primaries. For instance, the sun attracts both the earth and the moon, and, as they are always either at different distances from the sun or lie in different directions from the sun, they will be differently attracted by the sun; and hence their relative motions will be disturbed. Thus arise the perturbations of the moon's apparent motion. These purturbations naturally divide themselves into several classes; and they had been discovered from observation and divided into these classes, long before the theory of gravitation was invented. One of the first triumphs of the theory was the complete explanation of these classes of perturbation of the moon; and the suggestion of many others, which have been verified by the observations made since that time with more accurate instruments.

Of these applications of the theory of gravitation to explain the different perturbations of the moon, a great deal might be said. It is a subject involved in mathematical perplexity beyond anything else that I know. But there is one perturbation of the moon which is of so singular a character that probably I

may be able to give you some notion of it. It is that which is called the Moon's Variation.

In Figure 59, suppose E to be the earth, M′M″M‴ M'''' the moon's orbit, and C the sun. The sun, by the law of gravitation, attracts bodies which are near with greater force than those which are far distant from it. Therefore, when the moon is at M′ the sun attracts the moon more than the earth, and tends to pull the moon away from the earth. When the moon is at M‴ the sun attracts the earth more than the moon, and therefore tends to pull the earth from the moon, producing the same effect as at M′ or tending to separate them. When the moon is at M″ the force of the sun on the moon is nearly the same as the force of the sun upon the earth, but it is in a different direction. If the sun pulls the earth through the space E*e*, and if it also pulls the moon through the space M″*m*, these attractions tend to bring the earth and the moon nearer together, because the two bodies are moved as it were along the sides of a wedge which grows narrower and narrower. Thus, at M′ and M‴ the action of the sun tends to separate the earth and the moon, and at M″ and M'''' the action tends to bring the earth and the moon together.

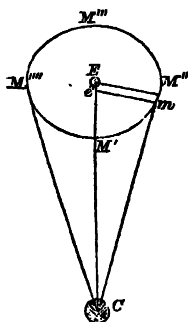


FIG. 59.

You might perhaps infer from this that the moon's orbit is elongated in the direction M′ M‴. No such thing: the effect is exactly the opposite. The fact really is, that the moon's orbit is elongated in the direction M″ M''''. And if you consider what has been

said before about the curvature of the orbit, and examine the subject for yourselves, you will see that it must be so. The moon I will suppose is travelling from M'''' to M'. All this time the sun is attracting her more than the earth, and therefore increasing her velocity till she reaches M'. When she is passing from M' to M'' the sun is pulling her back, and her velocity is diminished till she reaches M''. From this point her velocity increases again till she reaches M''' and then diminishes again till she reaches M''''. Therefore, when the moon is nearest to the sun, and furthest from the sun, she is moving with the greatest velocity; when she is at those parts of her orbit at which her distance from the sun is equal to the earth's distance from the sun, she is moving with the least velocity. I mentioned in a former lecture (see page 106) that the curvature of the orbit depends on two considerations: one is the velocity; and the greater the velocity is, the less the orbit will be curved: the other is the force; and the less the force is the less the orbit will be curved. The consequence is this; that as the velocity is greatest at M' and M''', and the force directed to the earth is least (because the sun's disturbing force there diminishes the earth's attraction,) the orbit must be the least curved there. At M'' and M'''' the velocity has been considerably diminished; the force which draws the moon towards the earth is greatest there (because the sun's disturbing force there increases the earth's attraction), and therefore the orbit must be most curved there. The only way of reconciling these conclusions is by saying that the orbit is lengthened in the direction M'' M''''; a conclusion opposite to what we should have supposed if we had not investigated closely this remarkable phenomenon. It will easily be understood that

the amount of this effect is modified in some degree by the change which the earth's attraction undergoes in consequence of the change of the moon's distance, (the earth's attractive force varying inversely as the square of the moon's distance) but still the reasoning applies with perfect accuracy to the kind of alteration which is produced in the moon's orbit.

This particular inequality was discovered by Tycho Brahé before gravitation was known; and it underwent an examination and was explained by Newton as a result of gravitation. There are other perturbations even more important than this, (the Progression of the Apse, the Evection, the Annual Equation,) of which I only mention the names; they were discovered before gravitation was known, and they were most fully and accurately explained by gravitation.

The next point which I shall mention is this: that the planets disturb one another generally. For it is to be remarked, that the attraction of planets is not confined to the sun, although, in consequence of the sun's very great magnitude and very great attraction, accurate and long-continued observations may be necessary for discovering the comparatively small effect of the planets. But the law of gravitation asserts that *every* particle of matter attracts *every* other particle, and therefore every planet attracts every other planet; and therefore the motions of the planets are not exactly the same as if only the sun attracted them. The differences of the real movements from the movements computed on the supposition that only the sun attracts the planets are the perturbations or disturbances of the planets. These disturbances are exceedingly complicated. In fact there is nothing in science which presents the degree of complication that these perturbations of the

planets and their satellites present. There is one kind of disturbances, however, of which possibly some notion may be given; they are the most remarkable in Jupiter and Saturn. There are many books, written as late as the beginning of the present century, in which the motions of Jupiter and Saturn are spoken of as irreconcilable with the theory of gravitation. It was one of the grand discoveries of La Place that the great disturbances of those two planets are caused by what is called the "inequality of long period," requiring some hundreds of years to go through all its changes.

Let Figure 60 represent the orbits of Jupiter and Saturn. You must observe that they are both ellipses, and the positions of their axes do not correspond. Now, the thing which La Place pointed out as regards these planets, affecting their perturbations, is one which applies more or less to several other planets; it is this: that the periodic times of Jupiter and Saturn are very nearly in the proportion of two small numbers, namely 2 to 5. Upon the proximity of that proportion depend entirely some of the peculiarities of their disturbances. And the effect of this will be seen if we consider in what part of their orbits their successive conjunctions will happen.

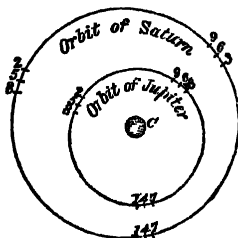


FIG. 60.

Inasmuch as these periodic times are in proportion of 2 to 5, it follows that when Saturn is describing two-thirds of a revolution in its orbit, Jupiter is describing almost exactly five-thirds of a revolution

in its orbit. And therefore, if the two planets have been in conjunction, then about twenty years afterwards Saturn has described two-thirds of a revolution, and Jupiter a whole revolution and two-thirds, and the planets will be in conjunction again, but not in the same parts of their orbits as before, but in parts more advanced by two-thirds of a revolution. Thus in Figure 60, 1, 1, to be the place of the first conjunction of which we are speaking. Saturn describes two-thirds of his orbit as far as the figure 2. Jupiter goes on describing a whole revolution and two-thirds of a revolution, and arrives at the same time at the figure 2 in his orbit, and the planets are in conjunction at 2, 2. Saturn goes on describing two-thirds of the orbit again, and comes to figure 3. Jupiter goes on describing a whole revolution and two-thirds of another, and he comes to figure 3, and they are in conjunction there. The next time they are in conjunction at figure 4, the next at figure 5, and the next at figure 6, and so on. These conjunctions occur in this manner from the circumstance that the periodic times are nearly in the proportion of 2 to 5; there are three points of the orbit at nearly equal distances at which the conjunctions occur.

But we will suppose that they occurred exactly at three equidistant points, and that time after time they happened exactly at the same points. It is plain that in that case there would be a remarkable effect of the disturbances, particularly at those parts of the orbit 1,1, 2,2, 3,3, &c., where Jupiter and Saturn are nearer to each other than at other times. They are very large planets, each of them bigger than all the rest of the solar system, except the sun; they exercise very great attractive force each upon the other; and therefore they would disturb each other,

in a very great degree and a very curious way, if their conjunctions occurred exactly at the same place.

Now these conjunctions do not occur exactly at the same place. The periodic times are nearly in the proportion of 2 to 5, but not exactly in that proportion. In consequence of the periodic times being not exactly in the proportion of 2 to 5, their places of conjunction travel on, until after a certain time the points of conjunction of the series 1,4,7, &c., would have travelled on until they met the series 3,6,9, &c. Not fewer than 900 years are required for this change.

Now so long as three conjunctions take place at any definite set of points, the effect on the orbits is of one kind. As they travel on, the effect is of another description (because, from the eccentricity of their orbits, the distance between the planets at conjunction is not the same), and so they go on changing slowly until the points of the series 1,4,7, &c., are extended so far as to join the series 3,6,9, &c.; and then the conjunctions of the two planets occur at the same points of their orbits as at first, and the effect of each planet in disturbing the other is the same as at first; and thus we have the same thing recurring over and over again for ages. During one-half of each period of 900 years, the effect that one planet has upon the other is that its orbit has been slowly changing, and then, during the other half, it comes back to the same thing again. Suppose that, during half the 900 years, one planet has been causing the other to move a little quicker, and that during the other half of that 900 years it has been causing it to move a little slower; although that change may be extremely small as regards the

velocity of the planets, yet as that velocity has 450 years to produce its effect in one way, and an equal time to produce its effect in the opposite way, it does produce a considerable irregularity. If the place of Saturn be calculated on the supposition that its periodic time is always the same, then at one time its real place will be behind its computed place by about one degree, and 450 years later its real place will be before its computed place by about one degree, so that in 450 years it will seem to have gained 2 degrees. The corresponding disturbances of Jupiter are not quite so large.

These are the most remarkable of all the planetary disturbances, their magnitude being greater than any other, on account of the magnitude of the planets, and the eccentricity of their orbits. There are, however, others of the same kind. One of these was discovered by myself; it depends upon the circumstance, that eight times the periodic time of the earth is very nearly equal to thirteen times the periodic time of Venus. I am afraid I have not conveyed to you any very definite notions of these things; but the foregoing is, I think, the best that can be done. In cases of this kind it is only possible to give a glimmering of what I desire to convey. I wish to impress upon your minds the fundamental circumstances on which these remarkable perturbations depend, and to what they tend, so that you may be able to think and in some measure to investigate for yourselves. I would observe that I have attempted to do all which I believe can be done in the way of popular explanation, in a book which I published some years ago, entitled *Gravitation*, which was re-published as an article in the *Penny Cyclopædia.*

I must however remind you, that I have attempted

to explain only one limited class of perturbations. There are some which may be described as a slow increase and decrease of the eccentricities of the orbits, and a slow change in the direction of the longer axes of the orbits; but there are others of which no intelligible account can be given to you.

In order, however, to bring these theories into actual calculation, it is necessary to know, not only the general tendency of the disturbances, but also their actual magnitude. In the perturbations produced by the earth, by Jupiter, and by Saturn, there is no difficulty in doing this. I have already shown you how we can calculate the number of miles through which the earth's attraction draws the moon in one hour. We are certain, from most careful experiments made by Newton and (in the present century) by Bessel, that the earth's attraction draws every body at the earth's surface through the same space in the same time; or in other words, that a ball of lead, a cricket ball, and a feather, will fall to the ground with equal speed, if the resistance of the air is removed. We say, therefore, that the earth's attraction would draw a planet through the same space as the moon, provided the planet were at the moon's distance; and for the greater distance of the planet, we must, on the law of gravitation, diminish that space in the inverse proportion of the square of the distance. Now I have already shown you how to compute the space through which the sun draws a planet in one hour; and therefore the problem now is, to compute the motion of a planet, knowing exactly how far and in what direction the sun will draw it in one hour, and also knowing exactly how far and in what direction the earth will draw it in one hour. Without pretending to explain to you

how this computation is made, it will be evident to you that we have thus the bases of accurate computation.

In like manner we can, from observations of Jupiter's satellites, compute how far Jupiter draws one of his satellites in one hour, and therefore how far Jupiter would draw a planet at the same distance in one hour; and then by the law of gravitation we can compute (by the proportion of inverse squares of the distances) how far Jupiter will draw a planet at any distance in one hour; and this is to be combined, in computation, with the space through which the sun will draw the planet in one hour. In like manner, by similar observations of Saturn's satellites, and similar reasoning, we can find how far Saturn will draw any planet in one hour, and we can combine this with the space through which the sun would draw it in one hour. Thus we are enabled to compute completely the perturbations which these three planets produce in any other planets; and then comes the critical question. Do the planets' motions, as computed with these disturbances, agree with what we see in actual observation? They do agree most perfectly. Perhaps the best proof which I can give of the care with which astronomers have looked to this matter, is the following: the measures of distances of Jupiter's satellites in use till within the last 16 years had not been made with due accuracy, and in consequence the perturbations produced by Jupiter had all been computed too small by about $\frac{1}{50}$ part. So great a discordance manifested itself between the computed and the observed motions of some of the planets (especially the small asteroids whose orbits are between those of Mars and Jupiter, more particularly Juno), and also in the motions of one of

the periodical comets, called Encke's comet, that many of the German astronomers expressed themselves doubtful of the truth of the law of gravitation. I made, and continued at proper intervals for four years, a new set of observations of Jupiter's satellites, and I had the good fortune to find that the satellites were further from Jupiter than was supposed, that the space through which Jupiter drew them in an hour was greater than was supposed, and that the perturbations ought to be increased by about $\frac{1}{50}$ part. These measures of mine were verified by continental observers. On using the corrected perturbations, the computed and the observed places of the planets agreed perfectly.

For the perturbations produced by Mercury, Venus, and Mars, which have no satellites, we have no similar foundation for our computations; and here we can only go on a method of trial and error. For instance, assuming for calculation that one of these planets has as great a disturbing power as the earth, we can compute how much it will disturb another planet's motion in every position; and if the disturbing power be altered in any proportion, we know that the disturbance of the other planet's motion will be altered in that proportion. We therefore find by trial what must be the proportion to make the calculated place of the disturbed planet agree most exactly with its observed place; and then, having settled to our satisfaction the measure of the disturbing power of Venus, or Mars, &c., we can try in all subsequent observations whether it makes the computed places agree equally well. It is found that they do agree perfectly well.

I shall only add to this that the motions of our moon are sensibly disturbed by the planet Venus.

An irregularity, which had been discovered by observation, and had puzzled all astronomers for fifty years, was explained about two years ago, by Professor Hansen, of Gotha, on the theory of gravitation, as a very curious effect of the attraction of Venus.

We have thus a mass of irresistible evidence to prove that the attractions of the sun upon the planets and upon our moon, of the planets upon their satellites, and of the planets one upon another, do follow the law of gravitation. But now comes another question: how do we know that these attractions are produced by every particle of matter in each of these different bodies, as is asserted by the law of gravitation? To prove this, I must refer you to a totally different set of computations and observations. I must make a comparison of the results of theory with the facts of observation, in some of the cases in which it is necessary to consider one body as attracting separately every particle of another, or to consider every particle of one body as separately attracting another body, or to consider every particle as separately attracting every particle.

The first subject to which I shall allude, is the precession of the equinoxes and nutation, which are produced entirely by the attraction which one body (the sun or the moon) exerts separately upon every separate particle of the earth. Upon these I have already spoken, (page 175); and there will be no need for me to detain you further at present, because you will have been sufficiently aware that there is general conformity between the results which I obtained upon Newton's theory and the results obtained by actual observation. With regard to the numerical agreement, I shall make some remarks presently, when I speak of the mass of the moon.

I will now speak of the ellipticity of the earth; and this, it will be found, is a case in which it is necessary to consider every particle as attracting every particle. First of all, you will remember that when the hoop, Figure 23, is put in motion round the vertical spindle, it changes its form. Now in order to explain this, there is a term commonly used which I believe I have not in these lectures hitherto uttered; the reason is, that I do not like it; I allude to the term "centrifugal force." In order to explain why this hoop expands horizontally when it is whirled round the vertical axis, I must recal to your minds the first law of motion. The first law of motion as applied to the hoop is this: if the part *a* of the hoop is put in motion horizontally, it would go on in a horizontal *straight* line if it could. No matter what may be the nature of the force which puts *a* in motion, it has no tendency to move in a circle; and if it were set free, as a stone from a sling, it would immediately fly off in a straight line. And by motion in a straight line, it would go further and further still from the central bar. In order to keep it at the same distance from the central bar, a restraining force is necessary. The term "centrifugal force" has been used to express the tendencies of the various parts of that hoop to acquire greater distances from the central bar. It is a bad term, because in reality there is no force. Perhaps it would be better to say "centrifugal tendency," tendency to recede from the centre, which will in all cases require a force to control it. Now this centrifugal tendency tends to change the figure of the earth; but the consideration of the centrifugal tendency alone is not sufficient to give us the means of calculating what the form of the earth will be.

Newton was the first person who made a calculation of the figure of the earth, on the theory of gravitation. He took the following supposition as the only one to which his theory could be applied: he assumed the earth to be fluid, or at least to be so far fluid in all parts below the surface that its form would be the same as if it were entirely fluid. This fluid matter he assumed to be equally dense in every part, so that it was composed of no heavier matter at the centre than at the circumference. For trial of his theory, he supposed the fluid earth to be a spheroid; he then computed the attraction of the whole spheroid upon every one of its component particles of fluid; with this he combined the centrifugal tendency; and then he examined whether, by giving a proper degree of ellipticity to the assumed spheroid, the forces computed on this supposition would be such as would keep the fluid in the spheroidal form which he had supposed to be the earth's form. Now upon the theory of gravitation it is evident that the attraction of a sphere is not the same thing as the attraction of a spheroid. It is necessary to compute what the attraction of this spheroid is, before we can enter into the effect of its combination with the centrifugal tendency. This is the result: suppose that the spheroid AB, Figure 61, is not revolving at all; still even in that case the attraction of the spheroid upon a body at the part A of the earth is greater than the attraction upon a body at the part B of the earth. But besides this, when we suppose the earth to revolve round the axis A*a*, there is the centrifugal tendency of which I have spoken, which does not affect the body at the part A of the earth in the axis of rotation, but which affects the body at B at a great distance from the axis of

rotation. We have to consider then that at the Poles of the earth there is an attraction which may

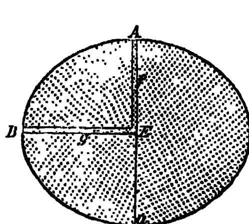


Fig. 61.

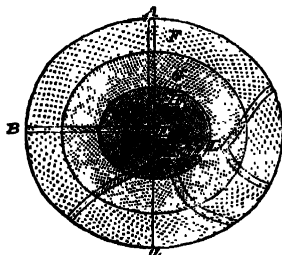


Fig. 62.

be computed when we assume that the earth is in the form of a spheroid; and at the equator there is an attraction which may also be computed, and is found to be smaller than that at the Pole, and which is still further diminished by the centrifugal tendency.

Thus the whole effective attraction at the Pole is sensibly greater than the whole effective attraction at the equator. This is not unfrequently expressed by saying that "a body weighs more at the Pole than at the equator." And this statement is correct, if it be received with the proper caution. If we carried a pair of scales with proper weights from the Pole to the equator, the same weights which balanced a stone at the Pole would balance it at the equator, because the effect of gravity on both is altered in the same degree. But if we carried a spring-balance from the Pole to the equator, the spring would be more bent by the weight of the same stone at the Pole than at the equator. There is also another

effect, to which I shall shortly allude, that a stone would fall further in one second at the Pole than at the equator.

Having computed the effective attractions at the Pole and at the equator, we must now examine what is the consideration to be applied in order to discover whether, with a certain supposition of ellipticity of the earth, this homogeneous fluid will be in equilibrium. The way in which Sir Isaac Newton proceeded is the same as that adopted by every other person who treats of the theory of fluids. You may conceive a cylindrical tube AE, open at both ends, to be put down from the Pole to the centre. Suppose you put down a similar pipe BE from the equator to the centre; and suppose that they communicate at the centre E—these imaginary pipes will not at all disturb the state of rest of the fluid, if it be at rest—by means of each of these pipes we shall ascertain the state of pressure of the fluid at the centre E. By the "state of pressure" I mean the measure of that *compression* of the fluid at E, which would enable it to burst any shell that enclosed it at E, if there were no opposing pressure on the outside of the shell; and this measure is to be understood as expressed by so many pounds per square inch; just as we measure the pressure of water in the cylinder of a Bramah's press, by so many pounds per square inch, meaning by that, the pressure on every square inch of its case, tending to burst it.

Now in order to find the pressure produced by the fluid in the column AE, it is not sufficient to know the length of that column, but we must also know the attraction which acts on every part of it. In ordinary cases we speak of the pressure of "a head of water," and we measure it by the depth of the

water, and that measure is accurate, because gravity acts equally on all the water in such depths as we have to treat of in ordinary cases; but if there were any part of the water on which gravity did not act at all, that part would add nothing to the pressure; or if there were any part on which gravity acted with only half its usual force, that part would contribute only half its proportion to the pressure. We must therefore ascertain, not only the lengths of different parts of the columns of fluid AE and BE, but also the proportions of the attractions acting on those different parts.

Now we have just seen that the attraction, as diminished by the centrifugal tendency, is less at B than at A; and I may now state as a result of mathematical investigation, that the attraction diminished by the centrifugal tendency is less at the middle of EB than at the middle of EA, and so at every corresponding part of their lengths. Therefore when we estimate the pressure produced by the fluid in the column AE, we have to consider that there is a short column of fluid, of which every part is pulled downwards by a large attraction; and for the column BE, we have to consider that there is a longer column of fluid, of which every part is pulled downwards by a smaller attraction. And therefore (on the principles which I have just explained in reference to a head of water) by choosing a proper ellipticity for the spheroid, or in other words, choosing a proper proportion of length of the columns AE and BE, the pressures per square inch which these columns produce at the centre E *may* be made exactly equal.

And now comes into account the fundamental property of fluids, namely, the equality of pressure

in all directions. When the fluid is in a state of rest, the pressure per square inch at E *must be* the same, whether it is estimated by the pressure which it exerts in sustaining the column EA, or by the pressure which it exerts in sustaining the column EB. This is the fundamental property of fluids, upon which (as a matter of science) I shall not speak at greater length; I shall merely remind those who have to do with steam boilers, or Bramah's presses, or other engines in which fluids are in a state of violent compression, that there is equal tendency to burst upwards, downwards, or sideways. Therefore when we have gone through the investigation, taking into account what is the attraction of the spheroid on every part of the fluid in each column, what is the amount of the centrifugal tendency in each part of the column in EB, and what is the length of each of the pipes, on an assumed ellipticity of the earth; and when we have thus found, by considering each pipe separately, the pressure at E where the two pipes join, we must have the two pressures equal. And if the ellipticity which we have assumed for the earth will not make these two pressures equal, we shall know that is a wrong ellipticity, and we must try another, till we find one which will make the pressures equal. If we use a proper algebraical process, we can diminish the trouble of this so far as to reduce it all to a single trial; but still the principle of the process is exactly the same. And when we have found one ellipticity which does make the pressures equal, we are sure that we have got the right ellipticity for the earth; still limited, however, by our original supposition, that the earth is a fluid of equal density in every part, and is not more dense at the centre than near the surface; and if that

original supposition be wrong, our conclusion will be wrong.

It was in this manner, assuming the earth to be a fluid no more dense at the centre than near the surface, and proceeding in every way as I have described, that Sir Isaac Newton inferred that the form of the earth would be a spheroid, in which the length of the shorter diameter or axis on which it turns, is to the length of the longer or equatoreal diameter, in the proportion of 229 to 230. And this, perhaps, may be considered as one of the most wonderful investigations in modern science.

With this proportion, as you will have perceived, is intimately connected the proportion of gravity at A and B, that is to say, of the attraction at A to the attraction diminished by centrifugal tendency at B. You must accept it as a result of mathematical investigation, of which I can give you no further explanation at present, that if you compare a point f in the line EA, with a point g in the line EB, such that Ef : EA :: Eg : EB, you then find the whole effective force at f, to bear the same proportion to the whole effective force at g, which that at A bears to that at B. You must also remember what I have said about the pressure at E depending not only on the length of the pressing column of fluid, but also on the effective force upon each part of it. Then you will easily see that the effective force or gravity at B, must be to the gravity at A, in the proportion of 229 to 230. All this depends on the supposition that the fluid is of equal density throughout.

The next point is, how can we verify this by observation? How can we find whether a body appears heavier upon one part of the earth than upon another? I have already said, that it will not

do to take a pair of scales and weigh the body with weights. The next suggestion is to weigh it with a spring-balance. It is not beyond possibility that a spring-balance might be made which would be sufficiently delicate for this purpose. The principal difficulty perhaps would be experienced in overcoming the effects of change of temperature in altering the elasticity of the spring: but if this could be done, and if the spring-balance otherwise could be constructed with very great delicacy, the gravity at different parts of the earth could be compared. But the method which actually is used for this purpose, is one depending on the effect of gravity in producing motion. Theoretically, this effect of gravity may be measured by observing how far a stone falls in one second; but practically it is more accurately ascertained by the use of a pendulum. This is susceptible of very great accuracy indeed.

The pendulum is made of metal; it turns with a hard steel prism, having a very fine edge, upon hard plates of agate, or some very hard stone. It swings like the pendulum of a clock. But you must observe that a clock pendulum will not do for this purpose, because there are other forces besides gravity acting on the pendulum, that is to say, the clock weights acting through the train of the clock wheels. It is necessary to have a detached pendulum. Now I wish to know how many vibrations that pendulum would make in a day. It is troublesome to find it out. Possibly the pendulum will not swing for a whole day.

In the experiments made in an expedition directed by the Spanish Government, a man was stationed on each side of the pendulum, to count 60 vibrations at a time; and they continued to count the vibrations

as long as the pendulum continued sensibly in motion. When they had got through a great number of 60's, they observed the time which a clock showed. It was a very tedious method indeed.

In every other instance in modern times, the vibrations of the detached pendulum have been compared with the vibrations of a clock pendulum. The mode adopted in the English and French expeditions was this: a detached pendulum is placed in front of a clock; a person is watching with a telescope; he watches when the two pendulums are going the same way; he remarks whether the vibrations of the detached pendulum recur quicker or slower than those of the clock pendulum; he sees that the vibrations separate more and more, till the two pendulums actually move in opposite ways; after this, they begin to move more nearly in the same way, and at length move exactly in the same way; perhaps the number of vibrations elapsed between these two agreements of motion may be 500. If you can determine the time when the two pendulums swing the same way, you find how long it is before one pendulum gains two vibrations upon the other. Then the calculation is this : suppose that the detached pendulum is going slower than the clock pendulum; and suppose that 7½ minutes elapse between two agreements of motion of the pendulum; then this shows that while the clock has gone 7½ minutes, or while its pendulum has made 450 vibrations, the detached pendulum has made only 448 vibrations. Now, the clock is going day and night, and by means of observation with the transit instrument, you can find how many hours, minutes, and seconds, the clock hands pass over in one day, or how many vibrations the clock pendulum makes in one day. Then, as the detached pendulum

makes 448 vibrations for every 450 made by the clock pendulum, you find at once how many vibrations the detached pendulum makes in 24 hours.

Some corrections for the effect of temperature in altering the length of the pendulum, and for other circumstances, are necessary; but I cannot enter upon the details of these at present. The method which I have described is exceedingly delicate. There is no difficulty in ascertaining by it the number of vibrations which the detached pendulum will make in a day, with no greater error than one-tenth of a vibration, or with an error not exceeding one eight-hundred-thousandth part of the whole.

This same pendulum is then carried to different parts of the earth, and is observed at every place in the same manner and with the same accuracy. The most important of our modern expeditions were those conducted by Colonel Sabine and Captain Foster. Each of these officers was entrusted by the Government with a ship, for the purpose of going to different parts of the earth, in order to observe the same pendulums at different places. Colonel Sabine went as far as a point in Spitzbergen, near the Pole; and both officers went to many places near the equator; to the West India Islands, to South America, and to South Africa. In this manner it was found that the number of vibrations which a pendulum makes per diem is not the same in different parts of the earth. When near the Pole, the pendulum makes about 240 vibrations in a day more than when near the equator. It is easily seen that this is a consequence of the force of gravity being greater there. If gravity be very small indeed, the motion of the pendulum will be exceedingly sluggish. The greater is the force of gravity which acts upon a pendulum when out of its

central position, the more briskly it pulls the pendulum down towards its central position, and the shorter time is occupied by every vibration, and the greater is the number of vibrations made in one day. Thus we have the means of measuring the gravity at different parts of the earth.

Now, the practical inference from the experiments is this : the proportion of the force of gravity at the Pole to the force of gravity (that is, attraction diminished by the centrifugal tendency) at the equator, is not as 230 to 229, as Newton stated, but is very nearly the proportion of 180 to 179. Now, here we have a remarkable departure from Newton's results. He proved that, if the earth were of equal density throughout, the proportion of the two axes would be as 229 to 230 ; and the proportion of gravity at the Pole and the equator would be as 230 to 229. We find, from trigonometrical surveys, and observations with the Zenith Sector, as you may remember, that the proportion of the earth's axis is as 299 to 300 ; and we have now found, from experiments with the pendulum, that the proportion of gravity at the Pole to gravity at the equator, is as 180 to 179. This shows that Newton was wrong.

The question then is, in what was he wrong? Now, it must be remarked, that Newton's calculation was founded entirely on the supposition that the earth is of equal density throughout. When we consider the matter, it is very unlikely that, if the interior of the earth is fluid, its density is equal in every part. Accordingly, in the last century, investigations were made, supposing the earth to be of different densities in different parts, and specially that the density increases as we approach nearer to the centre. The principal investigation (to which, in fact, nothing

important has been added in later times) was made by an eminent French mathematician, named Clairaut. The supposition on which he went (and which is really the only kind of supposition to be made at all in this investigation) was, that the earth consists of strata of different densities, but that each stratum is in some degree elliptical; the ellipticity of one stratum being different from that of another; and the investigations leave these ellipticities to be determined by considerations connected with the equilibrium of fluids.

For instance, in Figure 62, setting aside the water and floating matter on the top; suppose that F is the region of lava, if you please, that G is the region of melted iron, and that H is the region of melted platinum. Suppose you conceive one tube to be drawn from the Pole, and another from the equator, meeting at E; you make this a condition, that when you have investigated properly the gravity, (arising from the attraction of every particle of the spheroid upon any one particle, and modified by the centrifugal tendency from the axis *Aa*,) acting on the different substances at the different parts of these tubes, and when you have found the pressure of the fluids in the various tubes, by taking into account these several circumstances—the gravitation, with the centrifugal tendencies, the lengths of the different portions of the pipes, and the density of the fluid in each of those portions of the pipes on which the forces are acting; when you have taken these into consideration, you find the pressure of the fluid at the place E where the two pipes meet; then, by the principle of the equality of the pressures of fluids in all directions, you must have the two pressures at E equal, or the fluid will not be in a state of rest. Suppose then, that we have assumed such a degree of ellipticity for the external

surface of the earth, and such ellipticities for the different strata, that this condition of equality of pressure at E is satisfied; still we have not done all that is necessary. It is necessary that, if we suppose two or more tubes of any shape whatever, drawn from any points of the surface to any point of the fluid, as for instance the point K, or the point L, in Figure 62, the pressures at K produced by the fluids in the different tubes abutting at K shall be equal; and similarly, that the pressures at L produced by the fluids in the different tubes abutting at L shall be equal.

These considerations make the problem rather complicated. However, it can be completely solved, whatever be the succession of densities of the different strata; and the result is this. According to the law of density of the successive strata, the law of the ellipticities of the successive strata would be different, and the amount of the ellipticity of the earth's surface would be different. Except you know what is the structure of the interior, you cannot say what the ellipticity of the earth will be; but whatever that law of internal structure may be, you can say that there is a certain relation between the ellipticity of the earth and the degree of alteration of gravity from the Poles to the equator. Suppose you take a vulgar fraction to express the proportion of the whole diameter by which the earth is flattened at the Poles, and suppose you take another vulgar fraction to express the proportion of the whole gravity by which the gravity is diminished as you go from the Poles to the equator; if you add the two fractions together, whatever be the succession of densities of the different fluid strata of the earth, the sum of those two fractions will be $\frac{1}{115}$. This particular value $\frac{1}{115}$ depends upon the velocity of the earth's rotation: if the earth revolved in

a longer or shorter time than 24 hours of sidereal time, the sum must be a different quantity.

We are enabled thus by the pendulum experiments, which give us the law of change of gravity, to infer what is the ellipticity of the earth, provided the law of gravitation be true, (for that has been the basis of the whole investigation). When, by means of the pendulum, we have got the variation of the law of gravity, we have only to express it by a fraction, and to subtract that fraction from $\frac{1}{115}$, and we get another fraction which expresses the compression of the earth, or the difference between the two axes divided by one of them. And if the compression or ellipticity of the earth which we find by this process (depending entirely on the law of gravitation) agrees with that which we find from trigonometrical surveys and the use of the Zenith Sector, (in which the law of gravitation is not concerned at all,) this will be a strong proof of the correctness of the law of gravitation. Now, the proportion of gravity at the Poles and the equator is found to be about 180 : 179, so that the diminution of gravity in going from the Poles to the equator is about $\frac{1}{180}$ part. And if we subtract the fraction $\frac{1}{180}$ from the fraction $\frac{1}{115}$, the remainder scarcely differs from $\frac{1}{300}$, showing that according to this theory the ellipticity of the earth ought to be $\frac{1}{300}$, or the proportion of the earth's diameters ought to be as 300 : 299. And this is exactly the same proportion which has been found from triangulation surveys and Zenith Sector, as described to you in a former lecture. This, therefore, is a very remarkable proof of the correctness of the theory of gravitation, when applied with proper attention to all the circumstances.

There is another very curious method of determining the ellipticity of the earth, which also depends

upon the theory of gravitation. I have said that the attraction of a spheroid upon any external body is not the same as the attraction of a sphere; and therefore the attraction of the earth upon the moon is not the same as if the earth was a sphere. There is therefore a small irregularity in the motions of the moon depending on the earth's ellipticity; and it is very remarkable that, whatever be the succession of densities of the strata of the earth, this irregularity is found upon the theory of gravitation to depend upon nothing but the ellipticity of the earth's surface. And therefore, if we observe the moon's motions so carefully as to discover the amount of this irregularity, and if we make the proper calculation from it, we can find the ellipticity of the earth. The ellipticity thus determined agrees well with that found from the surveys: and thus another proof is given of the correctness of the theory of gravitation. It may not be amiss to state here that the motions of Jupiter's Satellites are much disturbed by the ellipticity of Jupiter's body.

There is another inference from these theoretical investigations of the figure of the earth, which it is proper to mention. Though we do not know what the law of the earth's internal structure is, yet we can assume some law of densities of successive strata gradually changing from the surface to the centre which shall give a value for the earth's ellipticity agreeing with the results which I have mentioned: and from this law we can find what the mean density of the earth is. The inference was thus made by Clairaut and his successors, that the mean or average density of the earth is about twice as great as at the surface, and that in some parts at and near to the centre, the density must be considerably greater than that mean density. Remarking that the mean density of the

earths and rocks at the earth's surface, taking one with another, is about twice and $\frac{6}{10}$ that of water, it was inferred that the mean density of the earth is more than five times the density of water. After this, another experiment was made, applying to the determination of the earth's mean density.

I have mentioned the liberality of George the Third in supplying funds for the observation of the transit of Venus. The same monarch (as I believe) supplied the funds for another experiment of great importance; it was the Schehallien experiment. Probably some of my auditors who have travelled in the highlands of Scotland, have seen the Schehallien Mountain; it may be observed from the banks of Loch Tay. If you go from Killin to Taymouth, it is on the left hand. Now this mountain was selected for observations of a very remarkable kind. It was argued that if the theory of gravitation were true, (that is to say, if attraction were produced not by a tendency to the centre of the earth, or to any special point, but to every particle of the earth's structure,) then by the fundamental law of gravitation, the attraction of a mountain would be a sensible thing; for a mountain is a part of the earth, with this difference only, that though the mountain is small in comparison with the earth, yet you get so close to the mountain, that its effect may be very sensible as compared with the effect produced by the rest of the earth. Some parts of the earth are 8,000 miles from us, and their attraction will be comparatively small. It was therefore thought worth while to ascertain whether the attraction of a mountain would be sensibly felt; and the Schehallien observations were a noble experiment towards the attainment of that result.

The Schehallien Mountain ranges east and west; it

was possible to make astronomical observations on the north and south sides; and it was also possible to connect the two places of observation by triangulation. Supposing Figure 63 to represent a section

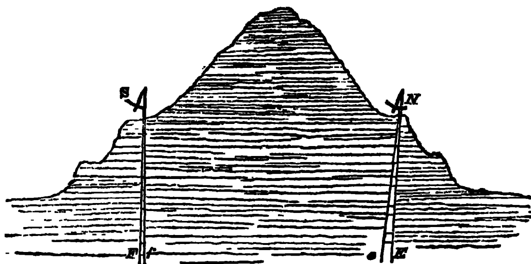


FIG. 63.

of the mountain north and south, N the northern, S the southern observing station. Observations were made at N and S upon stars with the Zenith Sector; the same instrument of which we have spoken so frequently, in reference to the determination of the elements of the earth's figure. By the use of the Zenith Sector, the difference of the directions of gravity at these two stations was found, exactly in the same manner as the difference of the directions of gravity in two stations of a meridional survey, Figure 18.

The direction of gravity at each station, you will observe, is the result of the gravity of the whole earth (as considered for a moment independently of the mountain), combined with the attraction of the mountain. And this is the consequence: supposing that at N, if there were no mountain, the direction of gravity would be NE; then introducing the

supposition of the mountain, the attraction of the mountain would pull the plumb-line side ways towards the centre of the mountain, and the direction of the gravity would be N*e*. And in like manner, supposing that if there were no mountain, the direction of the gravity at S would be SF; then, introducing the mountain, the effect of its attraction is to pull the plumb-line towards the centre of the mountain, and the direction of gravity would be S*f*.

Observe, then, the effect of the mountain; at N the direction of gravity is N*e* instead of NE, and at S, the direction of gravity is S*f* instead of SF; that is to say, the two directions which are taken by the plumb-line of the Zenith Sector, make a greater angle than they would if the mountain were not there.

Now, then, we come to the thing which we have to try. We know the general dimensions of the earth; we know what the inclination of the plumb-line at N and S would be if there were no mountain in the case. We know that this is a general rule: if we step 100 feet (nearly) forward, the direction of the plumb-line changes one second. If, then, we can find the distance from our observing station at N to that at S, then we can tell from that distance how much the directions of the plumb-line at N and S would be inclined if there were no mountain; and we can compare that inclination with the inclination observed by means of the Zenith Sector.

Accordingly, the observations were made in exactly the same manner as the observations made for determining the figure of the earth. The Zenith Sector was carried to N, and certain stars were observed; the Zenith Sector was then carried to S, and the same stars were observed at that place. By means of these observations of the stars, the actual inclinations

of the plumb-line at the two places were found. The next thing done was to carry a survey by triangulation across the mountain. This was done in the most careful way in which the best surveyors of the time could accomplish the task. The result was, that the distance between the stations was found such that, supposing that there was no mountain in the case, the inclination of the two plumb-lines ought to be 41 seconds. It was found practically from the observations by the Zenith Sector, that the inclination of the two plumb-lines actually was 53 seconds.

The difference between the two was the effect of the mountain. The mountain had pulled the plumb-line at one station in one direction, and at the other station in the opposite direction, to such a degree, that instead of the two plumb-lines making an angle of 41 seconds, they made an angle of 53 seconds; or, in other words, that the sum of the effects of the two attractions of the mountain, on opposite sides, was 12 seconds.

The next thing was, to draw from this observation a determination of the mean density of the earth. The general form of the process was this: the mountain was surveyed, mapped, levelled, and measured, in every way, so completely, that a model of it might have been made; it was then (for the sake of calculation) conceived to be divided into prisms of various forms: the attraction of every one of these was computed, on the supposition that the mountain had the same density as the mean density of the earth; and by means of this, the attraction of the whole mountain was found on the same supposition.

Thus it was found, that if the density of the mountain had been the same as the mean density of the earth, the sum of the effects of the attractions of

the mountain at N and S would have been about $\frac{1}{9933}$ part of gravity. But the observed sum of effects was 12 seconds, which corresponds to $\frac{1}{17,804}$ part of gravity. Hence the density of the mountain is only about $\frac{5}{9}$ of the earth's mean density; or the earth's mean density is nearly double of the mountain's density. The nature of the rocks composing the mountain was carefully examined, and their density as compared with that of water was ascertained; and thus the mean density of the earth was found to be something less than five times the density of water: a result agreeing nearly with that found from the assumption of the law of density of the earth's strata, connected with the observed variation of gravity, and observed ellipticity.

This was the nature of the celebrated Schehallien experiment, which was so extremely creditable to the parties by whom it was promoted and undertaken, and so important in its results.

After this another set of experiments was made; first by Mr. Henry Cavendish, a rich man, much attached to science, and who made many important contributions to chemistry, and other branches of natural philosophy (from whom the experiment of which I am speaking received the name of the Cavendish Experiment); afterwards by a Dr. Reich; and finally, in a very much more complete way, by Mr. Francis Baily, as the active member of a committee of the Astronomical Society of London, to whom funds were supplied by the British Government. It is an experiment of a different kind—a sort of domestic experiment—one of those experiments which can be made in your own observing rooms at home, and which are, in many respects, preferable to those made on the hill sides of Scotland.

The shape in which the apparatus is represented in Figure 64, is that in which it was used by Mr. Baily. There are two small balls A,B, (generally

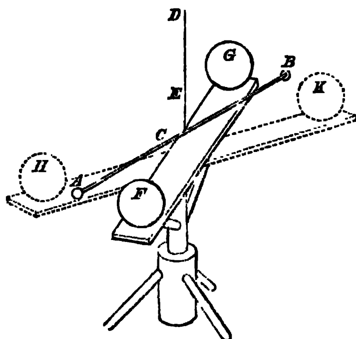


FIG. 64.

about two inches in diameter,) carried on a rod ACB, suspended by a single wire DE, or by two wires at a small distance from each other. By means of a telescope, the positions of these balls were observed from a distance. It was of the utmost consequence that the observer should not go near, not only to prevent his shaking the apparatus, but also because the warmth of the body would create currents of air that would disturb everything very much, even though the balls were enclosed in double boxes, lined with gilt paper, to prevent as much as possible the influence of such currents. When the position of the small balls had been observed, large balls of lead F,G, about twelve inches in diameter, which moved upon a turning frame, were brought near to them; but still they were separated from each other

by half-a-dozen thicknesses of deal boxes, so that no effect could be produced except by the attraction of the large balls. Observations were then made to see how much these smaller balls were attracted out of their places by the large ones. By another movement of the turning frame, the larger balls could be brought to the position HK. In every case, the motion of the small balls produced by the attraction of the larger ones, was undeniably apparent. The small balls were always put into a state of vibration by this attraction; then by observing the extreme distances to which they swing both ways, and taking the middle place between those extreme distances, we find the place at which the attraction of the large balls would hold them steady.

Suppose, now, the attraction of the large balls was found to pull the small balls an inch away from their former place of rest: then comes the question—what amount of *dead pull* does that show? The steps by which this is computed are curious.

First I must tell you that it has long been known (from experiment), that when a rod carrying balls is suspended in this manner by a wire, the space through which the balls will be pulled sideways is exactly in proportion to the force which pulls them sideways. In this respect, the law of forces acting on the suspended rod, is exactly similar to the law of forces acting sideways on a pendulum vibrating in a moderately small arc, for the motion of a pendulum is thus produced. If the pressure caused by the weight of the pendulum-bob, which acts vertically, is resolved into two parts, of which one part is in the direction of the pendulum rod, and the other acts sideways upon the pendulum, the former does not affect the movement of the pendulum at all, and the

latter, which produces the movement, is proportional to the distance of the pendulum from its place of rest, and therefore is similar in its law to the law of the force of twist of the suspending wire by which a rod with balls is supported (which force of twist is the same thing as the force which pulls the balls aside, because it exactly resists that force). Moreover, the force which acts sideways on the pendulum-bob, is in the same proportion to the whole weight of the bob, as the displacement sideways is to the length of the pendulum. Now the length of a pendulum which vibrates in a second, is 39·139 inches; and for such a pendulum, if it is pulled one inch sideways, the dead pull sideways (as I have just explained) will be $\frac{1}{39\cdot139}$ part of its weight: and thus we know that, for any balls or other things which vibrate in one second, the dead pull sideways corresponding to an inch of displacement is $\frac{1}{39\cdot139}$ part of their weight.

Then it is known as a general theorem regarding vibrations, that to make the vibrations twice as slow, we must have forces (for the same distances of displacement) four times as small; and so in proportion to the inverse square of the times of vibration. Thus if balls or anything else vibrate once in ten seconds, the dead pull sideways corresponding to an inch of displacement is $\frac{1}{3913\cdot9}$ of their weight. So that, in fact, all that we now want for our calculation, is the time of vibration of the suspended balls. This is very easily observed; and then on the principles already explained, there is no difficulty in computing the dead pull sideways corresponding to a sideways displacement of one inch; and then (by altering this in the proportion of the observed displacement, whatever it may be) the sideways dead pull or attraction

corresponding to any observed displacement is readily found. The delicacy of this method of observing and computing the attraction of the large balls may be judged from this circumstance: that the whole attraction amounted to only about $\frac{1}{20,000,000}$ part of the weight of the small balls, and that the uncertainty in the measure of this very small quantity did not amount probably to $\frac{1}{40}$ or $\frac{1}{50}$ of the whole.

Then the next step was this: knowing the size of the large balls and their distances from the small balls in the experiment, and knowing also the size of the earth, and the distance of the small balls from the centre of the earth, we can calculate what would be the proportion of the attraction of the large balls on the small balls to the attraction of the earth on the small balls (that is the weight of the small balls), if the leaden balls had the same density as the mean density of the earth. It was found that this would produce a smaller attraction than that computed from the observations. Consequently, the mean density of the earth is less than the density of lead in the same proportion; and thus the mean density of the earth is found to be 5·67 times the density of water.

The near agreement of this result with that found from the Schehallien experiment, and that found from the theory of the figure of the earth, (taking the observed ellipticity of the earth in combination with such a law of density as would produce that ellipticity,) shows, beyond doubt, that the same law of gravitation which regulates the attraction of the sun upon the planets, and the attraction of the earth upon the moon, does also apply to the attraction of a leaden ball upon another ball within a foot of it. In regard to the slight difference of results, it is probable that the result of the Cavendish experiment is the more

accurate of the two, simply because there is always some uncertainty upon the constitution of the rocks and mineral veins forming the interior of such a mountain as Schehallien.*

I have gone into this subject, "the evidence for the theory of gravitation," at some length. First, because *the law of gravitation is the most extensive in its application of all known laws*; for we are certain that it applies to every body, and to every portion of a body, in the Solar System, and we have strong reason to believe that it applies to the mutual action of those stars which are observed to revolve in binary systems, the two stars revolving each round the other. Secondly, because the explanation of the methods of calculating its effects, in several instances of varied character, leads us to the consideration of several very interesting principles and applications of them. And thirdly, because the assumption of this law is necessary for the estimation of the weight of the bodies of the Solar System, the part of my subject to which I now proceed.

I must first allude to the weight of the earth, because the weights of all other bodies of the Solar System are necessarily referred to it as a standard. Taking the dimensions of the earth as I have stated them before, the number of cubic miles in the earth is about 259,800,000,000; each cubic mile contains 147,200,000,000 cubic feet; and each cubic foot, upon the average, weighs 5·67 times as much as a cubic foot of water, or 354 lbs. 6 oz. avoirdupois. I will leave the combination of these numbers to you,

*** Another determination of the mean density of the earth has been made by the Astronomer Royal since these lectures were delivered. A short account of it is given in Appendix III.**

and will only remark at present, that I have shown you how the first step is made in referring the weights of the bodies of the Solar System, to the pound weight avoirdupois.

Next, I shall proceed with the estimation of the weight of the sun as compared with the weight of the earth. And this I shall do by comparing the attraction produced by the sun with the attraction produced by the earth at the same distance. And here is involved an important principle, namely, that the weight of a body is proportional to the attraction which it exerts. In order to explain this, it is necessary to remark that every calculation of perturbation in the Solar System requires us to suppose, that the attraction of one body A upon another body B is not a mysterious influence by which the presence of A causes a movement in B, without any reciprocal influence upon A, but is a real mechanical action which exerts equal strains upon both, just as if they were connected by a contracting spring. Thus, every strain which a large body A produces upon a small body B, is accompanied by an equal strain, produced by the small body B upon the large body A; and both A and B will be disturbed; but A will not be disturbed so much as B, because its mass is greater. In the computations of perturbations, for instance the perturbations of Saturn by Jupiter, it is necessary to consider that Jupiter attracts the sun according to the same law, (as regards the motion produced in it,) by which it attracts Saturn; else the computed disturbance of Saturn would not at all answer to the observed disturbance. If, then, the sun attracts Jupiter and a comet (when at equal distances from the sun) so as to produce the same motion in them, this shows that the mechanical pull upon Jupiter is greater than the

mechanical pull upon the comet in the same proportion in which the mass of Jupiter is greater than the mass of the comet; and therefore, (considering the reciprocal mechanical actions upon the sun as equal to the mechanical actions of the sun upon them,) Jupiter's pull upon the sun is greater than the comet's pull upon the sun in the same proportion as their masses; and the movements which they produce are in the same ratio. I may add, that the same principle is involved in every investigation relating to the figure of the earth. Assuming this principle then, I shall proceed to compare the attractions which the sun and the earth would exert upon a body at equal distances from them.

In former computations in this lecture, I found that in Figure 56, the earth draws the moon through 10·963 miles in one hour, the moon being at the distance of 238,800 miles from the earth; and in Figure 57, that the sun draws the earth through 24·402 miles in one hour, the earth being at the distance of 95,000,000 miles from the sun. In order to make these attractions comparable, we must reduce them both to the same distance; and we shall therefore first say, if the earth draws the moon through 10·963 miles in an hour when at the distance of 238,800 miles, how far would it draw the moon in an hour if it were at the distance of 95,000,000 miles? Diminishing 10·963 in the proportion of the inverse squares of the distances, we find that the earth would draw the moon through 0·00006927 mile or 4·389 inches in an hour, if it were at the distance of 95,000,000 miles. Comparing this with 24·402 miles through which the sun draws the earth or moon when at the same distance, we find that the sun's attraction is 352,280 times as great as the earth's, and therefore,

that the sun's mass is 352,280 times as great as the earth's. You can, if you please, combine this with the numbers which I gave before, to express the weight of the sun in pounds.

The angular diameter of the sun, as viewed from the earth, is 32 minutes of a degree. Computing from this the sun's diameter, we find that the sun's bulk is 1,400,070 times as great as the earth's bulk. Therefore the sun's mean density is only about ¼ of the earth's mean density, or about 1·4 times the density of water.

The principle which has been used above for comparing the mass of the earth with that of the sun, is used without the smallest alteration for comparing the mass of Jupiter, Saturn, Uranus, or Neptune, with that of the sun; and in all cases where the satellites can be easily observed, it can be applied with very great accuracy. For those planets which have no satellites there is considerable uncertainty. The only way in which they are determined is by the perturbation of other planets. For instance, we see that in certain positions, the earth is disturbed by Mars a few seconds, say six or eight. We compute what would be the amount of perturbations if the planet Mars were as big, or half as big, as the earth, and we alter the supposition till we find a mass which will produce perturbations equal to those which we observe. Thus we go through a process which is one of trial and error. In this manner the masses of Mars and Venus are determined. That of Mars is not very certain; that of Venus is more certain; both because it produces larger perturbations of the earth, and because its attraction tends to produce a continual change in the plane of the ecliptic, which in many years amounts to a very sensible quantity. The mass of Mercury is

still very uncertain; lately attempts have been made to deduce it from the perturbations which Mercury produces in the motion of one of the comets.

There is, however, one mass which is more important than the others, and that is the mass of the moon. There are several methods by which the mass of the moon is determined. In speaking of the precession of the equinoxes and nutation, I pointed out that lunar nutation is, in fact, an inequality of lunar precession, connected with it by a certain proportion which is known from the theoretical investigation. Therefore, as we can observe the amount of lunar nutation, we can, by taking that proportion backwards, compute the annual amount of lunar precession; and we can observe the whole annual precession produced by both the sun and the moon; and, subtracting the lunar part, there remains the part due to the sun. Thus we have got the proportion of lunar precession to solar precession. Now, you may remember, that on a former occasion, I went through the steps of a calculation, showing how, if we assumed the proportion of the moon's mass to the sun's mass, we might find the proportion of the lunar precession to the solar precession. By going backward through the same steps, knowing the proportion of lunar precession to solar precession, we may find the proportion of the moon's mass to the sun's mass.

There is a second method by which the mass of the moon may be obtained, from the proportion which its effect (depending upon the difference of its attractions upon different parts of the earth) bears to the sun's effect (depending upon a similar difference); and that is, by comparing the tides at different times. Everybody knows that the tides follow the moon generally but not entirely. They do not follow the time of the

moon's meridian passage by the same interval at all times; and they are much larger shortly after new moon and full moon than at other times. From a careful examination of all the phenomena of tides, it appears that they may be most accurately represented by the combination of two independent tides, the larger produced by the moon, and the smaller produced by the sun; that at spring-tides these two tides are added together, and make a very large tide; but that at neap-tides the high water produced by the sun is combined with the low water produced by the moon, and the low water produced by the sun is combined with the high water produced by the moon, and thus a small tide is produced.

By comparing the spring-tides with the neap-tides, we can find the proportion of the effect produced by the moon to that produced by the sun. Now, the tides are produced, not by the whole attraction of the moon and the sun upon the water, but by the difference between their attraction upon the water and their attraction upon the mass of the earth, by which difference the moon (and similarly the sun) attracts the water nearest to it from the earth, and attracts the earth from the water which is farthest from the moon.

Still these forces undergo some very peculiar modifications in their actions which produce the lunar and solar tides, which in many cases alter them in proportions slightly different. Thus their tidal effects are nearly but not exactly in the proportion of their difference of attractions, of which I have spoken; but with proper investigation it is possible to find, from their tidal effects, the proportion of their differences of attraction. And when this is found, we have obtained a proportion of differences of attraction which are exactly the same as the differences of attraction

concerned in producing precession (of which I have already spoken); and, from knowing this proportion, and knowing the distances of the sun and moon, we can, in the same way, find the proportions of the masses of the sun and moon.

The third method is this. In Figure 65, suppose C to be the sun, E the earth, M the moon. I have spoken continually of the sun's attraction upon the earth and of the earth's revolution round the sun, as if the sun were the only body whose attraction influenced in a material degree the earth's movement. But in reality the moon also acts in a very sensible degree upon the earth. And the immediate effect upon the motion of the earth is found by proper investigation to be the following. Draw a line from E to M, and in this line take the point G, which is called the "centre of gravity," so that the proportion of EG to GM is the same as the proportion of the weight of M to the weight of E; or so that if E and M were like two balls fastened upon the ends of a rod, they would balance at G. Then investigation shows that the motion of the earth may be almost exactly represented by saying that the point G travels round the sun in an ellipse, describing areas proportional to the times, (according to Kepler's laws), and that the earth E revolves round the point G in a month, being always on the side opposite to the moon.

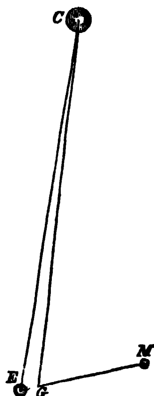


FIG. 65.

Consequently, the direction in which the earth would be seen from the sun (and therefore the

direction in which the sun is seen from the earth) depends in a certain degree on the distance EG. And therefore, if we observe the sun regularly, and if we compute where we ought to see the sun, according to Kepler's laws, the difference between these two directions will be the angle ECG; and knowing the distance CG, we can then compute the length of EG, and the proportion which it bears to GM; and this proportion, as I have said, is the same as the proportion of the mass of the moon to the mass of the earth.

A fourth method of determining the mass of the moon depends upon an accurate estimation of the force of gravity at the earth's surface. We know the moon's distance very accurately, and we know the earth's attraction at the earth's surface (that is, gravity) very accurately; and therefore we know the earth's attraction on the moon accurately. But the force which thus acts on the moon makes it revolve, not round E, in Figure 65, but round G. Now, in Figure 56, from a knowledge of the distance EM, and taking MN to be that proportion of the orbit which is described in one hour, we found the length of the line *m*N through which the earth's attraction pulls the moon in one hour. Here in Figure 65, we have the opposite problem; knowing the length of the line through which the earth's attraction pulls the moon in one hour, we have to find what is the length of GM, the semi-diameter of the orbit in which it revolves. Having found this, we find EG; and then, as in the last method, the proportion of EG to GM is the same as the proportion of the mass of the moon to the mass of the earth.

All these different methods agree very well in giving the result that the mass of the moon is about $\frac{1}{80}$ of the earth's mass. And when this mass, and the

known mass of the sun, are used in combination with a law of density of the strata of the earth which will well explain the observed ellipticity of the earth, it is found that they explain almost exactly the observed amount of precession.

There remains but one set of bodies whose masses can be determined; namely, Jupiter's satellites. It so happens that these little bodies disturb each other very much. In consequence of the periodic time of Jupiter's second satellite being very nearly double that of the first, and the periodic time of the third being very nearly double that of the second, there is a kind of "inequality of long period" in their motions which admits of tolerably accurate observation, by the observation of their eclipses. From this their masses are computed in the same manner as the masses of the planets from their mutual perturbations. Computations are made of the effect of one satellite upon the others, on the assumption, for instance, that the satellite is $\frac{1}{1000}$ part of the mass of Jupiter; if this does not produce, in calculation, the perturbations which are actually observed, the assumed mass must be altered in the proper proportion. For the fourth satellite, there are no perturbations of the nature of "inequalities of long period," but there are others sufficiently sensible, which are treated in just the same way. In this manner the masses of these distant little bodies are ascertained with reasonable accuracy. The largest of them (the third satellite) is about as large as our moon.

I have thus redeemed my pledge of explaining how the weights of the principal bodies of the Solar System are estimated by means of a pound avoirdupois. And I will here briefly recapitulate the principal steps.

First of all, I remarked that this estimation rests absolutely upon the truth of the Theory of Universal Gravitation, and I therefore pointed out the principal evidences of that theory. As these different evidences are nearly independent of each other, I shall not repeat them, but refer you back to what was said upon each of them.

Then the reference of weights to avoirdupois pounds begins with the weighing of the rocks of Schehallien for the Schehallien experiment, and the weighing of the large leaden balls for the Cavendish experiment; or, if you please, by weighing water, because the weights both of rocks and of lead are conveniently expressed by expressing the proportion which their weights bear to the weight of water.

The next step was this: by means of the Schehallien experiment and the Cavendish experiment, as well as by inferences from the ellipticity of the earth, we found that the mean density of the earth is between five and six times the density of water, and from that we were able to compute the weight of the earth.

The next step was this: having the dimensions of the moon's orbit round the earth, we could find how far the earth draws the moon in one hour; and having the dimensions of the earth's orbit round the sun, we could find how far the sun draws the earth in one hour; and comparing these, with the proper allowance for the difference of distances, we could find the proportion of the sun's mass to the earth's mass.

The masses of Jupiter and Saturn I explained to be found by ascertaining, from the dimensions of the orbits of their satellites and their periodic times, how far they draw their satellites in one hour; and then

comparing this space with the space through which the earth draws the moon, or the sun draws a planet, in one hour, only making the proper allowance for difference of distances.

For the masses of the other planets, I explained that there is no method but by the disturbances which they produce in the Solar System; and that these are made available by computing with an assumed mass what the perturbations would be, and altering the mass till these agree with the observed perturbations. Those of Jupiter's satellites, as I explained, are found in an analogous way.

For our moon, I indicated several different methods. One of these was, to infer (by theoretical considerations) from the observed amount of lunar nutation, what is the amount of lunar precession; to subtract this from the whole observed precession, which leaves solar precession; and thus to obtain the proportion of lunar precession to solar precession, which is the same as the proportion of the force with which the moon tends to pull the earth's surface from its centre to the similar force of the sun. A second method was from the proportion of lunar and solar tides, which is referred to the same proportion of forces as in the first method. A third method was, from the circumstance that it is not the earth, but the centre of gravity of the earth and moon, which moves very nearly in an ellipse round the sun. A fourth method was, that knowing the earth's attraction at its surface, and computing from this its attraction at the moon, we could infer from that the distance of the moon from the centre of gravity of the earth and moon. In the two latter methods we are led to an immediate comparison of the weight of the earth with that of the moon.

I shall now repeat what I said in commencing this course of lectures: that I fully believe that there is no part whatever of these subjects of which the *principle* cannot be well understood by persons of fair intelligence, giving reasonable attention to them; but more especially by persons whose usual occupations lead them to consider measures and forces; not without the exercise of thought, but by the application only of so much thought as is necessary for the understanding of practical problems of measures and forces.

APPENDIX.

I.

FOUCAULT'S PENDULUM EXPERIMENT.

In the year 1851, a method of rendering the earth's rotation visible to the eye was made known by M. Foucault, who had been led to discover it by considering the effect of the rotation of the earth on the apparent motion of a pendulum vibrating freely at the earth's surface.

If a heavy body, as for example, a sphere of metal be suspended by a string from a point A, Figure 66, vertically above N, the North Pole of the earth, and allowed to hang freely, the motion of the earth about its axis ANS will twist the string, and so cause the sphere to rotate about its vertical diameter. If, now, the sphere be drawn aside to a point B and allowed to drop gently, it will begin to vibrate in the plane NAB, and as the rotation communicated to the sphere does not tend to withdraw it from that plane, it will continue constantly to move in it. A spectator near N, partaking of the earth's motion, changes his position with reference to this fixed plane: but being unconscious that he is moving himself, he attributes to the fixed plane a motion exactly similar

to his own, but in the opposite direction. To him it will therefore appear to revolve from east to west

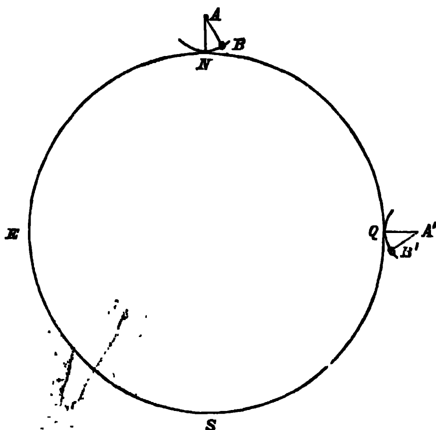


FIG. 66.

about the line NA, making a complete revolution in the course of a day. At the South Pole a similar appearance would be observed.

At places situated elsewhere on the earth's surface, it is less easy to anticipate the result; but some idea of the effect produced on the plane of vibration may perhaps be conveyed by the following explanation.

It has been remarked above, (page 108,) that a single force, acting in a given direction, may be *resolved* into two forces acting in given directions: and that these two forces acting together may be

regarded as producing the same effect as the single force acting alone. In like manner a single motion of rotation about a given axis may be *resolved* into two motions of rotation about two given axes: and if these two motions take place simultaneously, they may be regarded as together producing the same effect as the single motion. Thus, if a body (which for the sake of simplicity we may suppose to be

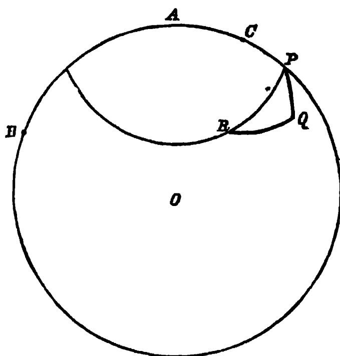


FIG. 67.

spherical) be made to rotate about the line OA, Figure 67, any point in it will describe a circle in a plane perpendicular to OA, with its centre on that line. Suppose that in a given time the point P is thus brought from one position P to another R; then it is possible to produce the same change in position by giving the body two successive rotations about two lines, OB, OC. For by virtue of a rotation about the line OB, P may be made to describe the

arc PQ of a circle having its centre on the line OB, and thus be brought to the position Q. Again, by a rotation about OC, Q may be made to describe the arc QR of a circle, having its centre on the line OC, and be brought to the position R. Thus, by two properly chosen steps, P to Q and Q to R, P is brought to R; and by giving the rotations about OB, OC, proper degrees of rapidity, *each* of the two steps may be taken in the same time as was the single original step, P to R. Suppose, now, that the two rotations, which we have hitherto supposed to be made one after the other, are made simultaneously: then the steps will, as it were, be taken together and in the same time as the single step; and P will, under the combined influence of the two rotations, be brought to the same position as it would be brought to under the influence of the single rotation. Thus, two rotations, about OB, OC, of proper degrees of rapidity, may be regarded as producing the same effect on every point of the body as does the single rotation about OA.

Now, if Q, Figure 66, be a place on the Equator, the earth has no motion of rotation about A′QE; and consequently the plane of vibration, having itself no motion of rotation, will appear to preserve a fixed position relatively to an observer at Q.

Again, let NOS, Figure 68, represent the earth's axis; let O be the centre of the earth and P a place on its surface; and let OR be a line perpendicular to OP and in the plane NPS, or the plane of P's meridian. Then, instead of supposing the earth's motion to consist of a single rotation about NOS, we may regard it as arising from two properly chosen rotations about OP and OR; and we may consider separately what would be the apparent

effects of these rotations on the pendulum : just as at page 111, the effects of the two components of a force are separately considered.

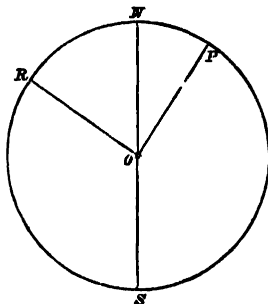


Fig. 68.

If, then, the earth rotated about OP alone, P would be one of its poles; and therefore, as has been already explained, to a spectator near P the plane of vibration would appear to revolve in the same time as the earth would revolve about OP alone, but in the opposite direction. Again, if the earth revolved about OR alone, P would be situated on the Equator; and a spectator would not observe any change in the plane of vibration. Thus at every place not situated on the Equator, the plane of vibration will appear to change its position; and such change will be entirely due to the component rotation about OP.

It is not difficult to submit these conclusions to an experimental test, and this has actually been done at many places. Care must be taken not to give the sphere any lateral motion at the moment

when it is dropped; otherwise, (referring to Figure 66,) it would at once be carried out of the plane BAN, and the previous reasoning would no longer hold. In practice, this object is attained by fastening the sphere to a wall or other fixed body by means of a second string, so as to keep it in such a position as AB; and then setting the second string on fire at an instant when the sphere is observed to be completely at rest.

II.

THE GYROSCOPE.

In general, a body which is set rotating about a line will not continue to revolve about it; on the contrary, the axis of rotation will usually change its position relatively to the body. Whatever be the shape of the body, however, it is always possible to find a line fixed relatively to the body, such that the body having once begun to rotate about it will continue to do so, provided no external force act: such a line is called a permanent axis of rotation. If the body be perfectly symmetrical about any line within it, that line is a permanent axis; thus the diameter of a sphere, the diagonal of a cube, and the axis of revolution of a spheroid, (page 60,) are permanent axes of the sphere, cube, and spheroid. If, further, when the body has once been made to rotate

about a permanent axis, no force intervene to alter the original motion, the axis will always preserve the same direction in space, as well as the same position in the body, a circumstance alluded to in the lectures, (page 78.)

Suppose, then, a body to be set rotating about a permanent axis and to be mounted so that the force of gravity does not interfere with the rotation; suppose, also, that at the commencement of the motion the axis points to some star; then, if it be true that the star does not move, the axis will always point to it so long as the rotation lasts. If it be true that the apparent diurnal motions of the stars are due to an actual motion of the earth; then to an observer on the earth's surface the axis will appear to move so as to follow the star. But if, on the other hand, the star move while the earth remains at rest, the observer we have spoken of will observe no change in the position of the axis. Thus we have the means of testing, by direct experiment, the truth of the conclusion arrived at in lecture II, (page 78,) that it is the earth which revolves and not the stars.

The following is the description of an instrument, contrived by M. Foucault, for the purpose of making such an experiment as we have just mentioned. DD′, Figure 69, is a heavy metallic disc, mounted on an axis which passes through O, the centre of the disc, and is perpendicular to its two sides. The extremities of this axis terminate in pivots CC′, which fit into holes made at opposite extremities of the diameter of a circular ring BCB′C′, which is furnished with two knife edges (similar to those of a balance) at B and B′, and so arranged that BB′ is the diameter of the ring perpendicular to CC′. The knife edges rest in holes made at opposite extremities

of the horizontal diameter of a vertical circle ABA′B′, which is suspended by a fine wire SA from the fixed point S. At A′, the opposite extremity of the vertical diameter AA′, is a pivot, which rests in a small hole. All the pivots are carefully polished, so that friction may be avoided as much as possible; and the dimensions of the different parts of the instrument are so adjusted that O is the common centre of the disc and the rings.

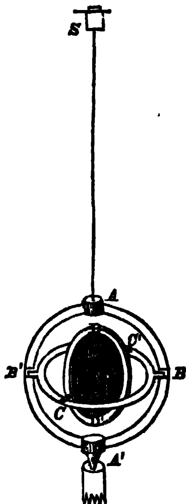


FIG. 69.

Now as the disc is symmetrical about CC′, that line is a permanent axis of rotation; and as O is the common centre of gravity of the different parts of the machine, and is supported by the string SA and the reaction at A′, the force of gravity will not interfere with the rotation. Thus the instrument satisfies all the conditions necessary for making the experiment. It is also clear, that the axis CC′ may be placed so as to point in any direction we please by moving first the ring ABA′B′, and then the ring BCB′C′ into proper positions.

To make the experiment, BCB′C′ is removed from its supports, and a rapid motion of rotation is impressed on the disc. The ring BCB′C′ is then restored to its place. It is found, that if at any

instant CC′ points to a fixed star, it continues to do so while the disc rotates, and thus appears to an observer to change its position relative to the surface of the earth; unless, indeed, the star be the pole star, in which case the observer will not notice any apparent change in its direction.

This instrument is called the "Gyroscope."

III.

THE DENSITY OF THE EARTH.

It is stated in the lectures, (page 222,) that if the law of universal gravitation be true, it is found (by a difficult mathematical investigation) that the attraction of the whole earth, considered as a sphere, on a body at its surface is the same as if the whole matter of the earth were collected at its centre. It is also found that the attraction of the earth on a body within its surface is the same as if the spherical shell situated between the body and the earth's surface were removed; or is the same as if all the matter situated nearer to the earth's centre than the body were collected at the centre, and all the matter situated at a greater distance were removed.

If the earth were of uniform density throughout, it would follow from these propositions that the force

of gravity at the bottom of a mine would be less than the force at the top. To shew this, suppose that the mine reached half-way to the centre of the earth. Then (since the volumes of spheres vary as the cubes of their diameters) the quantity of matter nearer to the earth's centre than the bottom of the mine would be only one-eighth of the whole quantity of matter in the earth. But the attraction of a quantity of matter at the earth's centre would be more powerful on a body at the bottom of a mine than on one at the top, in the inverse ratio of the squares of the distances of the bodies from the earth's centre: that is in the present case in the ratio of four to one. Hence the attraction on a body at the bottom of the mine would be, on the whole, less than the attraction on a body on the top in the ratio of one to two.

If, however, the earth be not of uniform density, but its density increase towards the centre, then though the attracting mass which acts on a body at the bottom of the mine be smaller, yet the diminution in the force of gravity so occasioned may be more than compensated by the comparative nearness of the attracted body to the denser parts of the earth. From the two laws of the attraction of spheres, which have been stated above, it is possible to calculate the ratio which the force of gravity at the bottom of the mine would bear to that at the top on any supposition we choose to make as to the ratio which subsists between the mean density of the earth and the density of the surface; so that if we know one ratio we can immediately infer the other. Now, pendulum observations afford us the means of determining the force of gravity at any place, (page 248,) and therefore, if the times of vibration of a

pendulum at the top and bottom of a mine be found, the ratio of the force of gravity at the top to that at the bottom may be calculated, and thence the ratio of the mean density of the earth to that of its surface.

This mode of determining the mean density was put in practice by the Astronomer Royal, at the Harton Coal Pit, near South Shields, in the year 1854. The mean density deduced from his observations is 6·565 : a value considerably exceeding that found from the Schehallien and Cavendish experiments.

THE END.

INDEX.

N.

O.

P.

R.

S.

COWELL'S STEAM PRESS, IPSWICH.

